Vol. XIX, No. 5 PSYCHOLOGICAL REVIEW PUBLICATIONS September, 1912

THE

Psychological Review

CONTENTS

PUBLISHED BI-MONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

41 NORTH QUEEN ST., LANCASTER, PA.

AND PRINCETON, N. J.

AGENTS: G. E STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.); LEIPZIG (Hospital St., 10); PARIS (76 rue de Rennes).

Entered as second-class matter July 23, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, Princeton University (*Index*)
JOHN B. WATSON, Johns Hopkins University (*Review*)
JAMES R. ANGELL, University of Chicago (*Monographs*)
ARTHUR H. PIERCE, Smith College (*Bulletin*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

THE PSYCHOLOGICAL REVIEW

containing original contributions only, appears bimonthly, on the first of January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

THE PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears the fifteenth of each month, the annual volume comprising about 480 pages. Special issues of the Bulletin consist of general reviews of recent work in some department of psychology.

THE PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The Index is issued in April or May, and may be subscribed for in connection with The Review and Bulletin, or purchased separately.

Annual Subscription to Review and Bulletin, $5.00 (Canada, $5.15, Postal Union, $5.30); Review, Bulletin, and Index, $5.85 (Canada, $6.00, Postal Union, $6.15); Bulletin, Alone, $2.75 (Canada, $2.85, Postal Union, $2.95).

Current Numbers of the Review, 50c.; of the Bulletin, 25c. (special issues 40c.); of the Index, $1.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The Philosophical Monographs form a separate series, containing treatises more philosophical in character. The Monographs appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $4.00. (Postal Union $4.30.) Each series may be subscribed for separately.

The price of single numbers varies according to their size. Thirteen volumes of the Psychological Monographs have been issued, and the first volume of the Philosophical Monographs is in progress.

LIBRARY OF GENETIC SCIENCE AND PHILOSOPHY

A series of bound books issued as accepted for publication. The price varies according to the size of the volume. Two volumes of the Library have already appeared.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY

Princeton, New Jersey

VOL. XIX. No. 5 September, 1912

THE PSYCHOLOGICAL REVIEW

INTROSPECTIVE ANALYSIS OF CERTAIN TACTUAL PHENOMENA [1]

BY GEORGE F. ARPS

Ohio State University

The introspections of this paper were obtained from two groups of pressure stimuli. In the first group the pressures were more or less extended; in the second group they were of momentary duration.

I. TACTUAL STIMULI OF MORE OR LESS EXTENDED DURATION

The data forming the basis of part of this paper were gathered in the laboratory at Leipzig, and some of these appeared in the November, 1908, *Psychologische Studien*.[2] The data under consideration here, being an integral part of that already published, were obtained under precisely the same conditions as given in the above-mentioned publication. A brief statement of these conditions follows:

Two tactual stimuli, a normal and a comparative, were applied successively to the upper phalanges of the index and middle fingers. The normal stimulus remained constant in intensity and varied in duration; the comparative stimulus, on the other hand, remained constant in duration and varied in intensity. The method of minimal changes was employed.

Two norms, 134.2 gr. and 58.2 gr. respectively, were used. Each of these norms had the following duration periods: 13^{σ}, 45^{σ}, 72^{σ}, 121^{σ}, 206^{σ}, 305^{σ}, 380^{σ}, 432^{σ}, 487^{σ}, 611^{σ}, 980^{σ}, $1{,}385^{\sigma}$

[1] A paper read before the American Psychological Association at the Minneapolis meeting, December, 1910.

[2] Wundt, *Psychologische Studien*, Vol. IV., pp. 431–471.

TABLE I

Duration of N.-S. 134.2 Gm.	13$^{\sigma}$	45$^{\sigma}$	72$^{\sigma}$	121$^{\sigma}$	206$^{\sigma}$	305$^{\sigma}$
Series of C.-S. in grams. Duration one sec.	10 (I) 15 20 (I°) 25 30 35 40(1′) 45	30 (I) 35 (II″) 40 (I′) 45 50 55(2″) 60(1°) 65	40 (I″)45 (III) 50 55 (I) 60(1′) 65(2°) 70(3) 75 80	(II°) 50 (I+I) 55 (V) 60 (I″) 65 70(5+3°) 75(3″) 80 85 90	60 (II″) 70 (III+II+I°) 80 90(1°) 100(1°) 110(3°+4′+5) 120(1)	(I°) 65 75 (III°+III) 85 95(3) 105(2″+3′) 115(2) 125

Duration of N.-S. 134.2 Gm.	380$^{\sigma}$	432$^{\sigma}$	487$^{\sigma}$	611$^{\sigma}$	980$^{\sigma}$	1,385$^{\sigma}$
Series of C.-S. in grams. Duration one sec.	70 (IV°) 80 (I′) 90 (III′) 100 110(2°+7) 120 130(1) 135	65 75 (I″) 85 (V″) 95(1) (I°) 105(4′+8+1″) 115 125(2′)	(II°) 75 85 (IV″) 95 (II) 105 115(5″+5°) 125(2″) 135(1)	80 90 (II) 100 (IV) 110 120(5) 130(2′) 140(2′) 150	(I′) 90 110 (II″) 130(3″) 150(4°) 170(1°) 190 210	85 (II′) 105 125 (II+III′) 145(1′) 165(5°) 185(3°) 205

TABLE I—*Continued.*

Duration of N.-S. 134.2 Gm.	13σ	45σ	72σ	121σ	206σ	305σ
Series of C.-S. in grams. Duration one-half second.	40 50 60 70 80(1) 90 100	50 60 70 80 90(2°) 100 110	60 70 (I) 80(1) 90(2′) 100 110(1) 120 130	(II°) 60 80(2) 100(2′+1″) 120 140 160	60 (III′) 80 100 (II″) 120(1) 140(1°+2) 160(1) 180	70 (II) 90(1) 110(2″) (II°+I) 130(2+1°) (II′) 150(1°+1) 170 190

Duration of N.-S. 134.2 Gm.	380σ	432σ	487σ	611σ	980σ	1,385σ
Series of C.-S. in grams. Duration one-half second.	70(2°) (II°) 90 110(5′) (IV) 130 150 (I°) 170(2°) 190 210	60 80 (I′) 100(3) 110 (I″+II′) 120(1′+1″) (IV″) 140(2) 160(2) 180	60 (I′) 80 100 (III°) 110 120(3+1′) 140 (I) 160(2) 180	70 90(1″) 110 130(2°+3) (I) 150 170 190(1°)	70 (II°) 90 110(2′) 130 (II+I°) 150(2″) 170 190(1°+1) 210	60 80 (III″) 100(1) 120(3) (I′) 140 160(2+1°) 180 200

The measuring, or comparative, stimulus had for one set of experiments a duration of one second; for a second set, one half second. The norms were at one time measured in terms of a comparative stimulus enduring for one second, at another time in terms of a comparative stimulus enduring for one half second, for each of their duration-periods.

The recurring complaint of the variable nature of the normal stimulus caused the writer to look carefully after the source of these apparent variances. All observers were asked to report any changes observed in the norm. Each change was properly noted in the series. That the phenomenon is not due to variations in the objective factors is shown by the carefully made tests at the time.[1] The explanation of the oscillatory nature of the norm is, therefore, to be sought in the subjective realm. An actual case will illustrate.

If a normal pressure of 134.2 gr. persists for a period of 380 sigmas and we measure its intensity in terms of a second pressure persisting for one second and if we use in this measurement a series of pressures of 70, 80, 90, 100, 110, 120, 130, 135, grams respectively, we observe that the norm when measured by 80 grams appears less intense than when the measurement has extended to a comparative stimulus of 110 grams or 130 grams. At 135 grams no assimilative effect is recorded. Frequently observer Franken maintained a gradual increment in intensity of the norm, paralleling the increment in the comparative series. In the above series, the norm subjectively increased as the comparative stimulus actually increased. What is true of an ascending series of intensities is, to a less degree, true of a descending one, in which latter case, 80 grams represents the lower terminus of assimilative effects. Contrast effects become manifest at the termini of the ascending and descending series when the intermediate members, 90, 100, 110, 120 grams are omitted. A summary may be had from Table I.

	(Norm 1st)	(Norm on index finger)
I = One	change reported in 1st time-order,	1st space-order, descending series.
	(Norm 2d)	
II′ = Two	changes reported in 2d time-order,	1st space-order, descending series.
		(Norm on middle finger)

[1] Arps, 'Uber den Anstieg der Druckempfindungen,' *Psychologische Studien*, Vol. IV., p. 467.

I°	= One change reported in 1st time-order,	2d space-order, descending series.
III″	= Three changes reported in 2d time-order,	2d space-order, descending series.
1	= One change reported in 1st time-order,	1st space-order, ascending series.
2′	= Two changes reported in 2d time-order,	1st space-order, ascending series.
1°	= One change reported in 1st time-order,	2d space-order, ascending series.
3″	= Three changes reported in 2d time-order,	2d space-order, ascending series.

The underlined figures represent the approximate positions, in the series, where the assimilative effects were observed to cease. No reports of norm variations were given above or below these points. It is extremely difficult to state definitely the exact points where the "attraction" between the two stimuli begins or ceases. Theoretically, it may be said to be coequal in extent with the series of comparative stimuli. The small arabic figures, enclosed in parentheses to the right of the tabular figures, signify the number of times and at what point in the ascending series, variability in the norm was reported. The Roman numerals to the left have a similar significance for a descending series. For a norm enduring 13 sigmas, the observer reported two changes when the comparative stimuli were given in the descending order (from 45 to 10 grams) and one change when given in the ascending series (from 10 to 45 grams).

It will be observed that the effect of one stimulus upon the other was confined to an upper and lower limit, above and below which assimilative effects ceased.

The number of introspections indicates that the greatest variability in the normal stimulus lies between the time periods 121 and 487 sigmas inclusive; it reaches its maximum at 432 sigmas. A very wide difference in the intensity of the two comparable stimuli is unfavorable to norm variability. On the other hand, a gradual increment of measuring stimulus, by a subtle insinuation, appears to draw the norm above or below its real intensity.

Deductions from the Tables

1. It appears that the comparative stimuli under the given conditions render constancy in intensity in a given norm subjectively improbable.

2. The assimilating elements (comparative stimuli) are most

efficient within certain limits of a series, above and below which assimilative effects are minimal or entirely lacking.

3. Greater assimilative effectiveness is found when the normal stimulus precedes the comparative stimulus (1st time-order). This is true for both ascending and descending series of comparative stimuli for the second and half-second periods of duration. Out of a total of 299 apparent norm variations, 190 were noted when the normal stimulus preceded the comparative stimulus (1st time-order) and 109 variations when the normal stimulus followed the comparative (2d time-order). It is probable that this is attributable to the fact that in the first time-order the assimilating element is an immediately present sensation, and, therefore, more efficient than the assimilated element (normal stimulus) which in this time-order is a memory element.

Deductions from Curves

1. There is a noticeable increment in assimilative efficiency when the comparative stimuli are given in the ascending series. That is, it seems more difficult to secure a subjective decrease in the intensity of the norm than a subjective increase. A total of 120 changes are noted in the descending series to 179 in the ascending series.

2. It appears that the assimilative efficiency, within certain limits, is proportional to the duration of the norm and of the comparative stimulus. For the normal stimulus the optimal period of duration for maximal efficiency lies between 121 and 487 sigmas; for the comparative stimulus the optimal period is one second.

3. It appears that some momentum on the part of the comparative stimuli is necessary to induce assimilation. This is evidenced from the fact that changes for the ascending series are more numerous at the upper limit of the series, while those for the reverse series are more numerous at the lower limit.

Ordinates indicate the number of norm variations; abscissæ indicate norm durations.

I. ---------- Curve of norm fluctuations accompanying a descending series of intensities. (C.S. 1 sec.)

III. — — — — — — Curve of norm fluctuations accompanying a descending series of intensities. (C.S. ½ sec.)
II. ———————— Curve of norm fluctuations accompanying an ascending series of intensities. (C.S. 1 sec.)
IV. — — — — — — — Curve of norm fluctuations accompanying an ascending series of intensities. (C.S. ½ sec.)

What are the influences at the bottom of this illusion? It is important to note here that the observers, without exception, regarded the variable comparative stimulus with much keener interest than that of the norm. It is not unlikely that the perception of change in the comparative stimulus grafted itself to the norm, and was a contributing factor in the illusion. In the normal procedure of the experiments a change in the intensity of the comparative stimulus was expected. Each observer was apprised of the intensity-variation of the measuring stimulus and the time-variation of the norm. It is not only probable that a definite perception of changes in the one stimulus affects the other, but that the *expectancy* of change, in which attitude the observer finds himself, plays a part in the illusion, determining the tendency of the judgment.[1]

Assimilative effects of a somewhat different order are those pertaining to the duration of the comparative stimulus, which stimulus, in the opinion of all observers, varied in its duration. Numerous tests were made which showed that the objective factors were constant and that, therefore, the fluctuations must be subjective in origin. It is believed here that the *normal stimulus acts assimilatively on the comparative stimulus with reference to its duration, as does the comparative stimulus on the normal stimulus with reference to its intensity.*

The amount of experimentation is insufficient on this point. However, the records show a sufficient number of statements of variation in the time of the comparative stimulus to merit attention. Comparatively few variations are recorded for the periods preceding 121 and following 611 sigmas. When the normal pressure persists for 611 sigmas the time variations are most emphatic. It is doubtful whether the factors given as causing the intensity illusion on the part of the norm apply equally to the time illusion on the part of the comparative stimulus.

[1] Arps, 'Uber den Anstieg der Druckempfindungen,' Wundt, 'Studien,' Vol. IV.

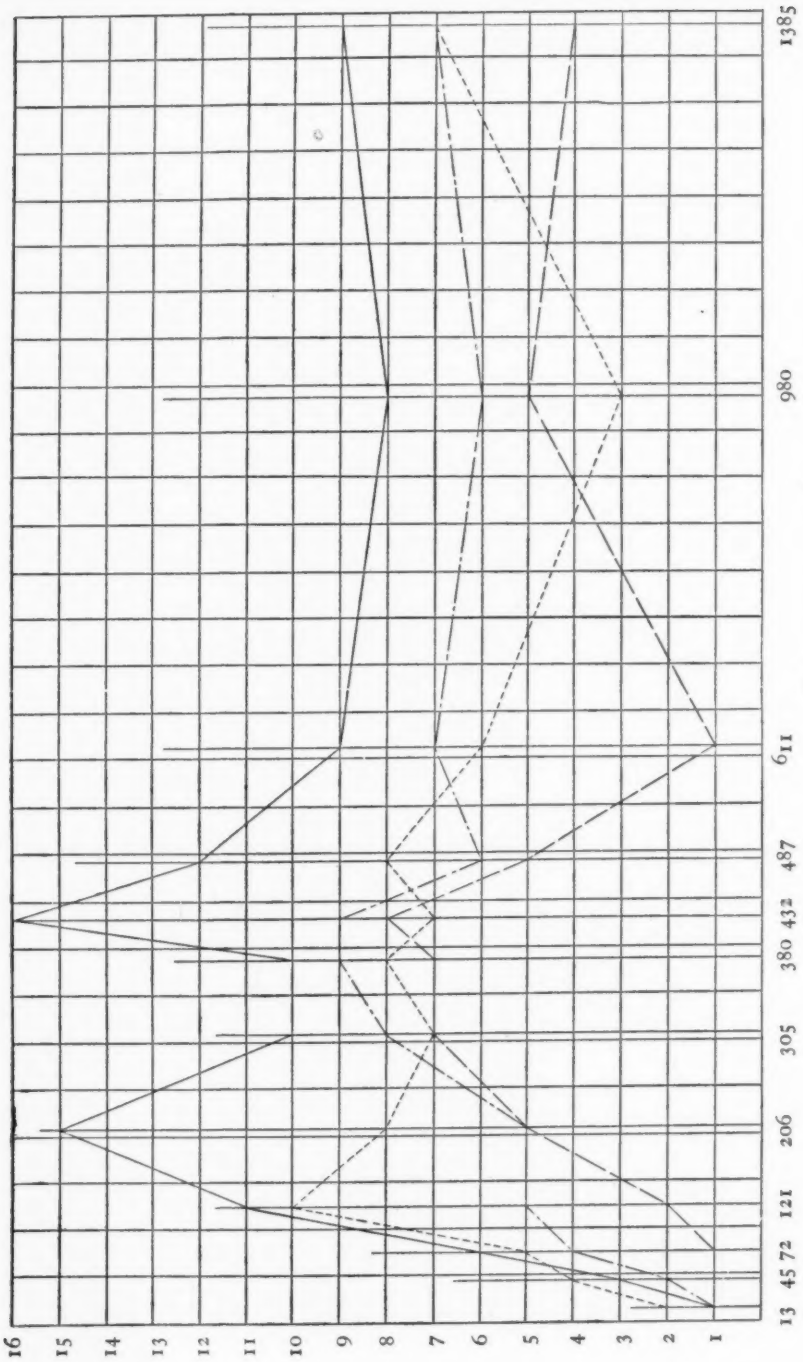

16
15
14
13
12
11
10
9
8
7
6
5
4
3
2
1
13 45 72 121 206 305 380 432 487 611 980 1385

FIG. 1.

The associated elements in this illusion have a somewhat different character from those involved in the illusion of intensity mentioned above. In the latter case are found only like or homogeneous attributes of intensity; in the former case are found the unlike attributes of intensity and duration. These unlike attributes are, however, of the same sense department, namely—pressure.

In just what manner the intensity of the pressure element associates itself with the attribute of duration is not clear. We may have here an intermediate form of assimilation—an association of only partially like elements. It is clear that the assimilation between the attributes of duration and intensity is not identical with the assimilation between the intensity of one pressure and that of another of the same kind. In the latter case the attributes are homogeneous throughout.

The work of Dr. Otto Klemm is interesting in this connection. He investigated the assimilative effect of light and sound. His method consisted in placing a tone stimulus in the median-plane of the observer about four feet distant and a light stimulus either to the right or to the left of the tone stimulus. It was so arranged that a tone of a given duration and intensity could be given successively or simultaneously with the light. Among the results the following are mentioned: (*A*) The light appears to draw the tone out of the median-plane, right or left, according to whether the light is placed to the right or left of the tone. (*B*) Increasing the distance of the light beyond a certain point causes a cessation of the assimilation. We observe here that assimilation is effective between disparate elements within certain limits. If we transgress beyond the above limits of assimilative effectiveness, we enter the field of contrast.

Another form of illusion of intensity is observed in the inclination of several observers to render a judgment before the close of the normal pressure (when the norm was given second) even before the beginning of it. Observer Franken says in this connection: "I carry the comparative stimulus over to the norm. Often a judgment is already formed upon the completion of the comparative stimulus. This judgment

is then either confirmed or rejected by the immediately succeeding normal pressure. It is usually confirmed. Should the normal pressure appear other than expected, the final decision is linked with a feeling of surprise and disappointment."

It appears that the absolute impression of the normal stimulus is especially potent in the comparison of the two stimuli. In an act of comparison as here described, factors, other than the sensation connected with the two pressures, enter.

There are a number of confirmatory investigations made along this line which point definitely to the presence of further conscious factors which aid in the formation of a judgment. The investigations of G. E. Müller and Lillian J. Martin,[1] shed some light on the factors involved in such a comparison. Martin and Müller after the method of Right and Wrong cases compare two weights, which are successively lifted. The results show that a judgment is reached in many cases, not on a direct comparison of the two impressions, but to a large extent on the absolute impression of heaviness or of lightness of the weights lifted. How do we gain this knowledge as to whether one of the lifted weights is light or heavy? In much the same way as we arrive at a knowledge in everyday life, whether a letter, a book, a trunk or a child is heavy or light, without comparing this object to some other definite object of the same kind. There is obviously here a factor, other than the immediate impressions of the two weights, which acts as a criterion for a judgment.

Other writers in other fields have observed mediate factors

[1] 'Beiträge zur Analyse der Unterschiedsempfindlichkeit,' pp. 44–45. "Unser Urtheil über die beiden gehobenen Gewichte beruht in vielen Fällen nicht auf einer Art von Vergleichung derselben, sondern stützt sich nur auf den absoluten Eindruck des einen derselben. Und zwar wird unser Urtheil, da es jedes Mal bei oder nach der zweiten Hebung abgegeben wird, selbstverständlich leichter durch den absoluten Eindruck des zuzweitgehoben Gewichtes bestimmt als durch denjenigen des zuerst gehobenen Gewichtes, der nur durch die Erinnerung auf das Urtheil zu wirken vermag. Macht das zuzweit (zuerst) gehobene Gewicht den absoluten Eindruck der Leichtigkeit, so haben wir eine Tendenz, das zuzweit gehobene Gewicht für kleiner (grösser) zu erklären als das zuerst gehobene; macht das zuzweit (zuerst) gehobene Gewicht den Eindruck der Schwere, so ist eine Tendenz vorhanden, das zuzweit gehobene Gewicht für grösser (kleiner) zu erklären als das zuerst gehobene."

in consciousness which purport to aid the connection between a complex of sensation and its expression in a judgment.

Stumpf and Meyer[1] investigated the sensitivity to discord (Verstimmungen). Two tones were given which at one time stood in octave relation, at another time approximated this relationship. The observer was to state whether the interval appeared pure, too large, or too small. Stumpf reached the conclusion that there are elements of consciousness other than the two primary tone sensations, which act as criteria for a judgment. For the lengthened interval such elements as "Unlustgefuhl" of tension, "Schärfe," "Überreizung" make themselves felt; for the shortened intervals an "Unlustgefuhl der Mattigkeit, Schalheit, Stumpfheit"; and for the subjectively pure intervals, a distinct feeling of pleasure.

From the introspections, it appears that a narrow definition of assimilation is untenable. Such definition cannot be limited, for example, to simultaneous associations or to associations of elements belonging to like compounds. The work of Klemm conclusively shows the assimilative effects between a given stimulus and a disparate one. We may group the various kinds of assimilation here observed into the following classes:

(1) Between homogeneous attributes, *i. e.*, intensity.

(2) Between disparate attributes, *i. e.*, intensity and duration.

(3) Between heterogeneous elements, as illustrated by the assimilation of sensory elements of disparate senses, *i. e.*, visual sensation and auditory sensation.

II. Tactual Stimuli of Momentary Duration

The infrequent manifestations of subjective fluctuations in the normal stimulus of extended duration (134.2 grams) for the briefer periods of exposure (13 and 45 sigmas) led to the belief that duration is an important factor in the production of such fluctuations. To this end pressure stimuli of momentary duration were given in a manner similar to the pressure stimuli of extended duration.

[1] *Zeitschrift fur Psychologie der Sinnesorgane*, 18, pp. 390–392.

Fig. 2 diagrammatically represents the manner of presenting stimuli of momentary duration. The pendulum, *p*, of the

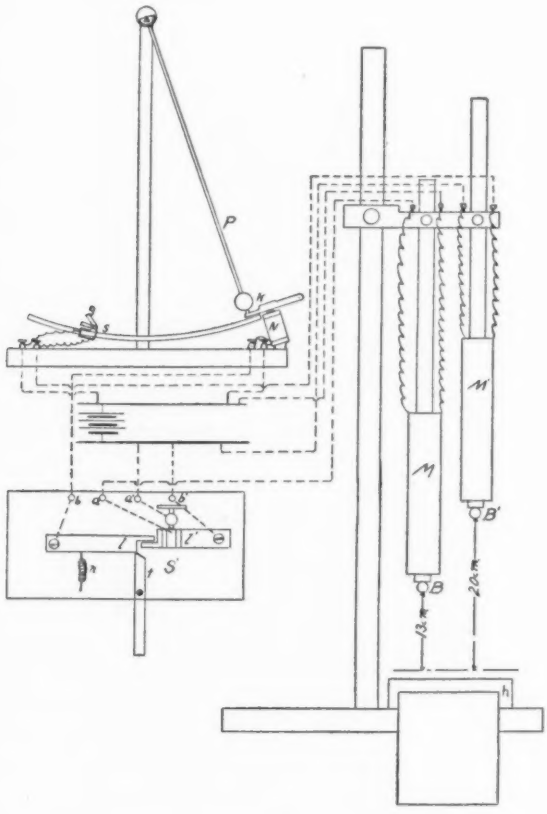


FIG. 2. Apparatus securing momentary pressure stimuli.

Bergström chronoscope is retained by a hook, *k*. The switch, *S'* (in position shown) is closed between *a* and *a'* and open between *b* and *b'*. In this position and with the contact made at *S* the electromagnets *M* and *M'* are receiving current.

The apparatus is set in function by pulling the trigger, *t*, to draw the arm, *l*, into contact with *l'*, by means of the spring, *n*, thus connecting *b* and *b'* and simultaneously breaking connection between *a* and *a'*. The electromagnet, *N*, now actuates the hook, *k*, releasing the pendulum and simultaneously break-

ing the current controlling the electromagnet, *M*, which releases the suspended ball, *B* (normal stimulus). The pendulum swings through its arc opening the switch, *s*, by striking the projecting arm, *g*. This breaks the current to the electro, magnet, *M'*, which releases the ball, *B'* (comparative stimulus).

Method

The standard stimulus consists of a small metal ball weighing 3.5 grams. To secure a variety of intensities the ball is dropped perpendicularly from three positions. These positions, from the magnet contact, *B*, Fig. 2, to the upper phalanges of the index finger, are 13, 27 and 40 cm. respectively. The right hand is placed, with special reference to these positions upon a rest, *h*. Owing to repeated complaints of pain, very few readings were taken from the 40 cm. position. These readings are consequently neglected.

The comparative stimuli, applied so far as practical after the method of minimal changes, consist of a series of eight small metal balls, designated by numbers reading from one to eight inclusive (Table II.). The weight of the various members of the series, following the order designated, is as follows: 1.4-2.2, 3.5, 4.5, 5.8, 7.4, 10.1, 13.6 grams. The distance through which the comparative stimuli drop remains constant at 20 cm. from the magnetic contact *B'* to the upper phalanges of the middle finger. The comparative stimulus follows the standard for the first and second position by an interval of 302 and 331 sigmas respectively.

Table II

Normal Stimuli 3.5 Grams

Variations in Norm First Position (13 cm.) for Increasing and Decreasing Series of Comparative Stimuli	Variations in Norm Second Position (27 cm.) for Increasing and Decreasing Series of Comparative Stimuli	Total Variations for Both Positions for Increasing and Decreasing Series of Comparative Stimuli
(I) 1 (1)	1	(I) 1 (1)
2	(II) 2 (1)	(II) 2 (1)
(II+1) 3	(I) 3	(III+1) 3
4 (2)	4 (1)	4 (3)
5 (1)	[2] 5	[2] 5 (1)
[2] 6	[1] 6 (1)	[3] 6 (1)
[1] 7	7 [1]	(1) 7 [1]
8	8	8

(1) = One increasing variation observed in the norm when the comparative stimuli are given in an increasing series of intensities.

(I) = One decreasing variation observed in the norm when the comparative stimuli are given in a decreasing series of intensities.

(I + 2) = One decreasing and two increasing variations observed in the norm when the comparative stimuli are given in decreasing series of intensities.

(2 + I) = Two increasing and one decreasing variations observed in the norm when the comparative stimuli are given in an increasing series of intensities.

[1] = One increasing variation observed in the norm when the comparative stimuli are given in a decreasing series of intensities.

[I] = One decreasing variation observed in the norm when the comparative stimuli are given in an increasing series of intensities.

The results are practically negative. Only 21 cases of norm fluctuations are recorded among a total of 1,622 experiments. Assimilative efficiency appears dependent upon the *duration* of the presented stimuli. Both assimilative and assimilated elements appear to require more than momentary duration to induce fluctuations in the subjective intensity of the norm. This is first evidenced by the introspections recorded for the normal stimulus of 134.2 grams with a duration period of 13 sigmas when measured by a comparative stimulus enduring for one second (Table I.). The norm in this case approximates momentary duration while the measuring stimulus endures for one second.

In opposition to the view here put forward it may be urged that the method of minimal changes is not sufficiently adhered to to bring about assimilative effects between stimuli of momentary duration. The series of balls composing the comparative stimuli fail to shade by imperceptibly small increments from one of the series to the next above or below it. The minimal obtainable difference between any two consecutive

members of the series is .8 gram; the maximum difference, neglecting the last, is 2.7 grams. The distribution of judgments, however, clearly indicates that the incremental values in the ascending and descending series of comparative stimuli here used in no way impairs the validity of the method employed. That the factor of duration conditions the above phenomenon of assimilation is therefore believed to be valid.

ESTHETICS OF SIMPLE COLOR ARRANGEMENTS

BY KATE GORDON

Mt. Holyoke College

The experiments reported below in part I. of this paper were performed in the psychological laboratory of Mount Holyoke College during the year 1905–6. Those reported in part II. were performed in the psychological laboratory of Columbia University during the year 1906–7. Since the further tests which I wished to make must be indefinitely postponed there seems no reason for delaying any longer the publication of these little studies.

I

The question which suggested these tests might be phrased as follows. "In massing colors on a canvas is there any general reason for placing certain colors near the center and others near the outside. For example, in combining red with blue may we consider that it is better usually to put the blue in the center and the red in the peripheral parts of the field, or the red in the center and the blue in the periphery?"

Preliminary tests were made with colors arranged as in Fig. 1. A central square of one color was surrounded by four

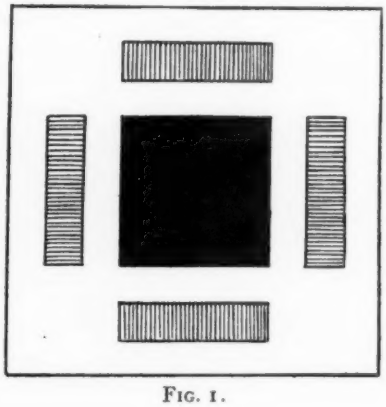


FIG. 1.

strips of another color, the spatial extent of the two colors being equal. This proved unsatisfactory for two reasons: (1) The figure as a whole was ungraceful and uninteresting to the subjects, and (2) the central color was disliked on account of its unbroken mass. The subjects found their attention repelled by the undifferentiated center, and it was evident that more complexity must be introduced into the figure to get an unquestioned esthetic reaction. The researches of Pierce[1] and Puffer[2] suggested that, since colors of different brightness were

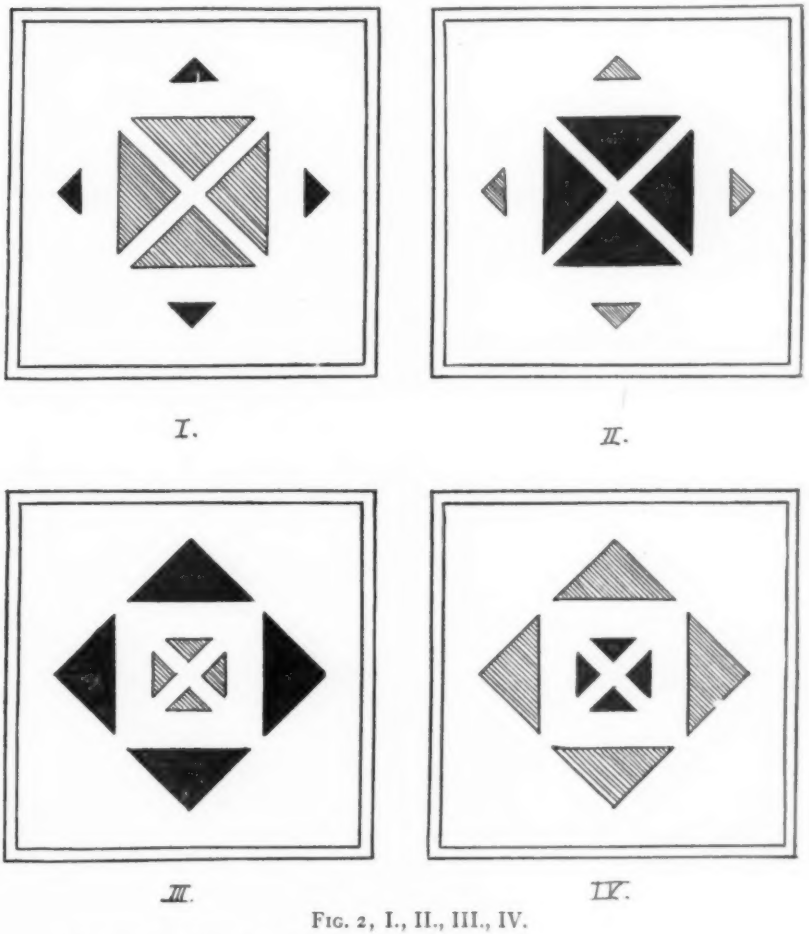


FIG. 2, I., II., III., IV.

[1] 'Æsthetics of Simple Forms,' PSY. REV., Vol. I.

[2] 'Studies in Symmetry,' Harvard Studies, Vol. I.

to be used, a contrast of large and small masses would be desirable. The figures finally chosen, Fig. 2, I., II., III. and IV., seemed the simplest ones which would meet the requirements. These four designs when filled out with a given color combination, *e. g.*, blue and red, represent the following possibilities of arrangement:

I. Small masses of blue in the periphery with large masses of red in the center.

II. Small masses of red in the periphery with large masses of blue in the center.

III. Large masses of blue in the periphery with small masses of red in the center.

IV. Large masses of red in the periphery with small masses of blue in the center.

The experiments were carried on in daylight illumination, and the colors used were the saturated hues of the yellow, green and blue of the Milton Bradley papers and the red of the Hering laid-on discs. The relative brightness of these colors may be stated in terms of the Hering gray paper series. The colors were matched with gray by indirect vision, and it appeared by this method that the

yellow equals in brightness Hering gray No. 2,
green equals in brightness Hering gray No. 8,
red equals in brightness Hering gray No. 13,
blue equals in brightness Hering gray No. 24.

The color combinations in these tests never included more than two colors at a time. This made only the following six combinations possible: blue-yellow; red-green; blue-red; green-yellow; blue-green; and red-yellow. All of these were used.

The method of presenting the combinations by paired comparisons was rejected after some trials. Anyone who has tried it with esthetic tests will recognize, I think, the serious objection against it, that it so quickly exhausts the esthetic reaction.

The figures were shown first upon a background nearly black, made of No. 45 Hering gray paper. This background paper, mounted on cardboard, was surrounded by a black frame 30 cm. square. The subject sat two meters away in

front of a shelf which was draped in gray. Upon the shelf were set, with interspaces of about 6 cm. the four frames containing the different figures. The subjects closed their eyes until the figures were all in place; they were then told to open their eyes and choose the most agreeable of the four designs. No restriction was made on the method of observing the figures, no fixation point was maintained and no time limit set. The subject was also asked to make a second and a third choice. Thus the task was to name the four designs in the order of preference. The frames were then taken down and the four figures filled out with another color combination. These tests were all repeated at a later sitting with this variation, that the figures were shown in reverse order on the shelf, namely 4, 3, 2, 1. Professor Martin[1] has pointed out the importance of relative position in the choice of simple figures, but since these figures were somewhat complicated and had individuality it was thought unnecessary to present them in each possible order, as 2 1, 4, 3; 3, 1, 2, 4, etc.

The subjects were twenty-nine young women in the junior and senior classes of Mount Holyoke College. All had had elementary work in psychology and several had served before as subjects of psychological experiments. They did not know the purpose of the tests and were told not to discuss their preferences with one another.

The method of tabulating the judgments was this: If a subject said that in the series, I., II., III., IV., No. III. was most agreeable, then that figure was credited with three points, because it was preferred to the three other figures. The second choice was marked two points because it was preferred to two others. The third choice was marked one because it was preferred to one other, and the fourth figure was marked zero. When the tests were repeated the figures were marked a second time, and the two sets of marks added. In cases where the subject was unable to choose the count was divided between the figures. Thus if the subject said that I. and II. were the best, two but she could not choose between them, each of these figures was given two and one half points, because first choice

[1] Psy. Rev., 1906, Fechner number.

counted three and second choice two. Counting up the preferences of all subjects in this way it appeared that with the dark background the preferences were distributed as in Table I.

TABLE I

Color Combination	Fig. I	Fig. II	Fig. III	Fig. IV
Blue-yellow:				
No. of prefs.	65.5	72	138	72.5
Percentage	18.8+	20.6+	39.6+	20.8+
Red-green:				
No. of prefs.	67	91	106	84
Percentage	19.2+	26.1+	30.4+	24.1+
Blue-red:				
No. of prefs.	72.5	77.5	118.5	79.5
Percentage	20.8+	22.2+	34.0+	22.8+
Green-yellow:				
No. of prefs.	75	72	126	75
Percentage	21.5+	20.6+	36.2+	21.5+
Blue-green:				
No. of prefs.	77.5	78	120.5	72
Percentage	22.2+	22.4+	34.6+	20.6+
Red-yellow:				
No. of prefs.	58	86	137	67
Percentage	16.6+	24.7+	39.3+	19.2+

In every color combination, then, the same figure received the greatest number of preferences, namely, the one in which small masses of bright color in the center are surrounded by large masses of darker color in the periphery. Moreover the combination which shows greatest disparity of brightness, the blue-yellow, shows greatest excess of preference for Fig. III. while the combination which shows least disparity of brightness, the red-green, shows least excess of preference for Fig. III.

In order to see whether these results would be modified by the brightness of the background against which the colors were shown all the above tests were repeated, substituting for the dark background a light one made of Hering gray No. I. The whole designs were then framed in light gray frames. The results are as given in Table II.

TABLE II

Color Combination	Fig. I	Fig. II	Fig. III	Fig. IV
Blue-yellow:				
No. of prefs.	40.5	100.5	132	75
Percentage	11.6+	28.8+	37.9+	21.5+
Red-green:				
No. of prefs.	65.5	87.5	110	85
Percentages	18.8+	25.1+	31.6+	24.4+
Blue-red:				
No. of prefs.	63	75.5	114	95.5
Percentage	18.1+	21.6+	32.7+	27.4+
Green-yellow:				
No. of prefs.	62.5	91.5	123.5	70.5
Percentage	17.9+	26.2+	35.4+	20.2+
Blue-green:				
No. of prefs.	57.5	86	126	78.5
Percentage	16.5+	24.7+	36.2+	22.5+
Red-yellow:				
No. of prefs.	48.5	95	138	66.5
Percentage	13.9+	27.2+	39.6+	19.1+

Adding together the whole number of preferences for each figure irrespective of color combinations there are

	Fig. I	Fig. II	Fig. III	Fig. IV
With dark ground	415.5	476.5	746.0	450.0
With light ground	337.5	536.0	743.5	471.0

The most noticeable difference lies in the relative increase of preference for II. over I. on the light ground.

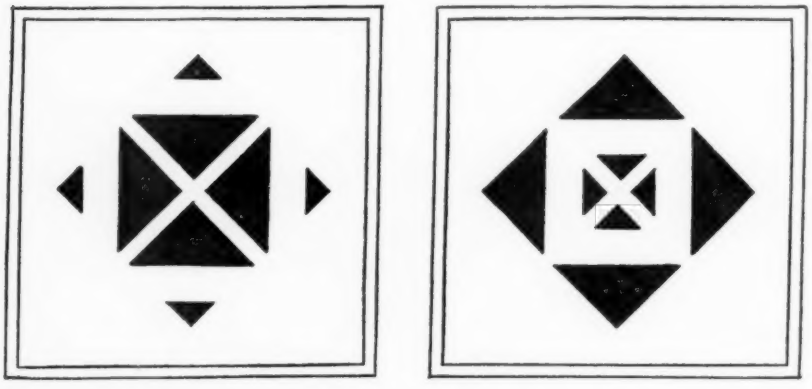
For the sake of isolating the several factors which contributed to the pleasantness of Fig. III. tests were next made to find out whether there was any preference for Figs. III. and IV. over I. and II. apart from the color combinations involved. The two figures were therefore shown side by side each filled out with a single color as in Fig. 3, I. and II. Here the color being constant the choice lay solely with the figure. It is conceivable, however, that the color chosen to fill them out with might affect the choice of the figure. Hence the two figures were filled out with each of the four colors in turn. These tests

were all repeated with the position of the figures reversed. Twenty-seven subjects took part.

TABLE III

Total No. of Prefs.	Fig. 3, I	Fig. 3, II
Filled out with blue	23	31
Filled out with yellow	20	34
Filled out with red	19	35
Filled out with green	21.5	32.5
Total of Prefs. on Light Ground	**Fig. 3, I**	**Fig. 3, II**
Filled out with blue	24	32
Filled out with yellow	21	35
Filled out with red	20.5	35.5
Filled out with green	26.5	29.5

There was therefore a constant preference for 3, II., as a figure, but, as Tables I. and II. show, this preference could be neutralized by other factors. The subjects who preferred 3, I.,



I. II.

FIG. 3.

said that it seemed more free and graceful than 3, II. Those who preferred 3, II., said that this one seemed more compact and unified.

The next step in the analysis of Fig. 3, III., was to abstract from the relative size of the peripheral and central masses, and to see whether the bright color is preferred in the center apart from any effect of large and small masses. Two designs were made in which the central and peripheral masses were equal in

extent. Fig. 4, I., shows the lighter color inside and 4, II., the darker inside. The results were, for twenty-nine subjects:

TABLE IV

With Dark Ground	Fig. 4, I	Fig. 4, II
Blue-yellow	38.5	19.5
Red-green	23	35
Blue-red	36	22
Green-yellow	38	20
Blue-green	39.5	18.5
Red-yellow	33	25
With Light Ground (15 Subjects)	**Fig. 4, I**	**Fig. 4, II**
Blue-yellow	20.5	9.5
Red-green	19.5	10.5
Blue-red	20.5	9.5
Green-yellow	20	10
Blue-green	19.5	10.5
Red-yellow	20	10

It appears, then, that with one exception (the case of the red-green combination on the dark background) the majority of preferences falls to the figure with the brighter color in the center. The reversal in the case of the red-green may be better understood in view of the results to be reported in part II. below.

For the sake of trying what effect a contrasting frame might exercise on these choices a series of tests was made in which the colors were shown on the light ground but surrounded by the black frames. Fourteen subjects took part, and the tests were repeated in reverse order.

TABLE V

Light Ground with Dark Frames	Fig. 4, I	Fig. 4, II
Blue-yellow	14	14
Red-green	12	16
Blue-red	14	14
Green-yellow	12.5	15.5
Blue-green	9.5	18.5
Red-yellow	9.5	18.5

The results show a striking reversal of judgments for the most part, *i. e.*, in every combination the figures with the dark color in the center are liked as well or better than those with the bright centers. The explanation I believe to be this: the rim

of black around the edge of the whole design makes a rhythm with the central color when this is a dark one. Only one subject, however, seemed consciously to realize the presence of

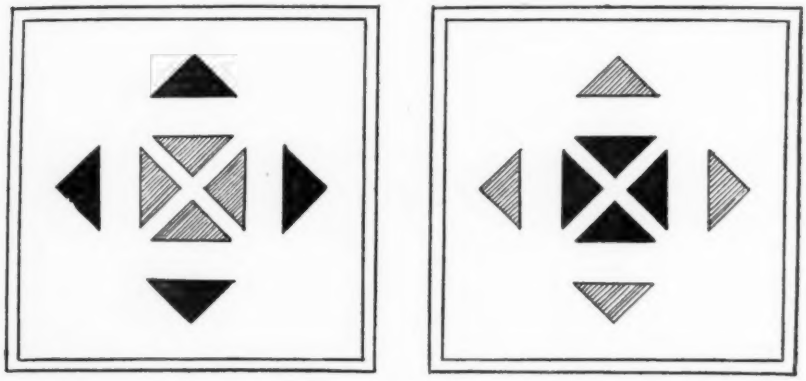


FIG. 4.

any such rhythms in the figures. No tests were made in which the dark ground was surrounded by a light frame. We must conclude that the brightness of the frame may have a very striking influence on the preferred color arrangement.

One further factor to be noticed is the preference for individual colors. One subject thought that she tended to prefer figures which had in them large masses of the favorite color irrespective of the arrangement. Each subject was asked to name the four colors used in order of their pleasantness (*A*) as seen on the dark ground, (*B*) as seen on the light ground. The color chosen first was counted three, the second two, etc.

RESULTS FOR 28 SUBJECTS

Dark ground	Red	Yellow	Green	Blue
	54	49	38.5	26.5
Light ground	Blue	Red	Green	Yellow
	60	47.5	44.5	16

Thus the most agreeable impression was made by the blue on the light ground, and the most disagreeable by the yellow on the light ground. Adding together the total choices, however, we get Red, 101.5; Blue, 86.5; Green, 83; Yellow, 65. Yellow

called out the widest extremes of choice. By some it was very much disliked, to others it was slightly disagreeable or indifferent, whereas a few said it was by far the most agreeable. It appears from these tests that the tendency to choose large masses of a favorite color is not a dominant tendency; for on the dark ground red was the best color, yet, in combination, small masses of red are chosen with large masses of blue and of green.

II

Seeing that the choice of colors as central seemed to be a function of their brightness, the next step was to equate the colors in brightness and to see what would then determine the choice. This part of the experiment was performed in a dark room. A light-proof box was made, inclosing an electric light. In front of the light was a ground glass plate which diffused the light over the designs which were fitted into the front of the box. The front of the box, which was 60 cm. wide and 30 cm. high, had grooves into which the cardboards carrying the designs in colored gelatines could be slid. The figures used were the same as Fig. 4, I. and II. above.

The colors used were the gelatine sheets furnished by the Stoelting Co. To match the colors in brightness two thicknesses of the blue gelatine were chosen as a starting point. The electric light was thrown through the glass plate and through the blue gelatines on to a screen at right angles to the light. At right angles to the screen a half-disc was set up which caught the direct light from a standard candle, the wick of which was kept as constant as possible. The candle could be moved until a point was reached at which, by the flicker method, the light from the candle was judged equal to the blue light. As a matter of fact the flicker never wholly disappeared with the blue light, but the minimum flicker, according to the middle judgment of three observers, occurred when the candle was 425 cm. distant from the half-disc. This, then, was taken as a standard, and the other colors were reduced to this brightness by the addition of sheets of gelatine. The judgments of two observers were taken for these equations and they coincided exactly. For the red, six sheets of gelatine

were necessary, for green, seven sheets, and for yellow ten sheets of gelatine and four sheets of yellow japanned paper. Of course the reduction of brightness by adding sheets of gelatine is not an ideally perfect system, because it does not give a continuous process, but it is only fair to say the the blending of the red, green and yellow with the candle light was very smooth and satisfactory, and if there was any objective difference in the brightness of the four colors it was very small, and, I think, negligible for the purpose of this experiment. The colors differed in saturation. Subjectively they all seemed pretty well saturated, but the spectroscope showed that, although red and green were good, the red lying between 640 and 620 on the spectrum scale, and the green between about 525 and 500, the blue transmitted violet, green and red, and the yellow transmitted green and red.

The experiment was conducted as above except that the subject sat in the dark until a signal was given and the electric light turned on which illuminated the colors from behind. As soon as the judgment of preference was given the light was turned off, and an interval of five minutes was allowed before the next pair of figures was presented. The subjects were all, with one exception, students in psychology at Teachers College. There were sixteen women and one man. The tests were repeated at a second sitting with the left and right position of the figures reversed. The results are shown in Table VI.

TABLE VI

Combination		No. of Prefs.
Blue-yellow:	Blue preferred as center	8
	Yellow preferred as center	26
Red-green:	Red preferred as center	24
	Green preferred as center	10
Blue-red:	Blue preferred as center	9
	Red preferred as center	25
Green-yellow:	Green preferred as center	12
	Yellow preferred as center	22
Blue-green:	Blue preferred as center	11
	Green preferred as center	23
Red-yellow:	Red preferred as center	23
	Yellow preferred as center	11

An examination of this table shows that in every color combination the color which is chosen as the center is the one which stands first in the order of the spectrum. Thus if we add together the total number of preferences for designs having red as a center we get 72, with yellow as a center 59, with green as a center 45, and with blue as a center 28.

I have no explanation to suggest for these preferences, but it seems probable that whatever factor was operative to determine choice in these last tests will serve to account for the case noted above in Table IV., where red was preferred over green as a center, though there the green was brighter.

Summary

1. When large and small masses of color appear together, it is more agreeable to find the large ones in the periphery of the visual field.

2. Brighter colors are preferred near the center of such figures, darker colors near the periphery, whether the background of the colors is light or dark.

3. In figures where the central and peripheral masses are equal in size, and where a light background is surrounded by a black frame, a dark color is preferred in the center.

4. There is probably some tendency to prefer large masses of a favorite color, but this tendency does not prevail over other considerations.

5. When colors are equated in brightness the color which stands nearer the red end of the spectrum is preferred in the center.

AN OPTICS-ROOM AND A METHOD OF STANDARDIZING ITS ILLUMINATION

BY C. E. FERREE AND GERTRUDE RAND

Bryn Mawr College

I. Introduction

In a previous article[1] the statement was made by the writers that all comparative estimates of the sensitivity of the retina to color (limens or limits) should be made in daylight instead of in the dark-room. This is to eliminate the influence of the field surrounding the colored stimulus, and of the preëxposure. When the surrounding field is black, white is induced by contrast across the stimulus color. Since the colors all differ in brightness,[2] the induction takes place in different amounts for the different colors. This white, in proportion to its amount, reduces the action of the colors on the retina. Further, a given amount of white affects to different degrees the action of the different colors on the retina. To eliminate this twofold unequal action, the surrounding field should be made in each case of the brightness of the color to be used. This can be done by working in a light-room of constant intensity of illumination and by making the surrounding field of a gray paper of the brightness of the stimulus color. In order to accomplish this and at the same time be able to work in any meridian of the retina we choose, we have constructed a special piece of apparatus which we call a rotary campimeter.[3] The influence of preëxposure is even more important than of sur-

[1] Ferree and Rand, 'A Note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units,' *Amer. Journ. of Psychol.*, 1912, XXIII., p. 331.

[2] In a later paper, one of the writers (Rand) will show that it is of no advantage to equate in brightness in determining the limits of color sensitivity, and that harm results in so many ways from the attempt to equate, that it is doubtful whether it should be done even in determining the limens of color in the more sensitive parts of the retina.

[3] See C. E. Ferree, 'Description of a Rotary Campimeter,' *Amer. Journ. of Psychol.* 1912, XXIII., pp. 449-453.

rounding field. If the preëxposure is to black, white is added as after-image to the stimulus color. The effect of a black preëxposure upon the stimulus color is greater than the effect of a black surrounding field because more white is added as after-image of preëxposure than is induced by contrast from the surrounding field. This effect also can be eliminated only by working in a light-room of constant intensity of illumination and choosing as preëxposure a gray of the brightness of the color to be used.

Standardization for either one of these factors, however, can be accomplished for one degree of illumination only.[1] As the general illumination changes, the relation of the brightness of the preëxposure and of the surrounding field to the brightness of the colored stimulus changes. It is obvious, then, that if standardization is to be accomplished with regard to the influence of either of these factors, some means must be devised of maintaining the general illumination of the retina constant. No satisfactory method has as yet been obtained for keeping the illumination of a room by daylight constant. To keep it constant presupposes what has not as yet been provided, namely, a sensitive means of measurement. Constancy may be approximated by artificial illumination, but no artificial source of light has yet been devised which gives a light that approaches average daylight[2] sufficiently closely in composition to warrant its use in color work. Of the various sources of light the Moore Tube comes nearest to doing this, but spectrophotometric and colorimetric determinations show that the light from it contains an excess of blue,[3] and, therefore, although it

[1] When the colored light used to stimulate the retina is independent of the general illumination, *e. g.*, when it is obtained from the spectrum, from monochromatic sources, or from standard filters, these two factors alone will modify the result of the color observation. If, however, light reflected from a pigment surface be used as stimulus, a change in the illumination will in addition change the amount of colored light coming to the eye.

[2] For results of measurements of the color values of average daylight, see Nichols, E. L., *Transactions of the Illuminating Engineering Society*, 1908, III., p. 301. Ives, H. E., 'The Daylight Efficiency of Artificial Illuminants,' *The Illuminating Engineer*, 1909, IV., pp. 434–442; and 'Color Measurements of Illuminants,' *Transactions of the Illuminating Engineering Society*, 1910, V., pp. 189–207.

[3] See Ives, H. E., 'Color Measurements of Illuminants,' *Transactions of the Illuminating Engineering Society*, 1910, V., p. 206; and Rosa, E. B., quoted by Moore,

has been adopted by various textile concerns for use in color matching, its substitution for daylight can scarcely be recommended for the more exact requirements of color optics. Ives and Luckiesh[1] attack the problem of producing artificial daylight from another side. By their subtraction method they claim to have gotten the closest approximation to average daylight yet attained. They aim to cut out by absorbing screens the excess of red and yellow in artificial light due to the comparatively low temperature of artificial illuminants. Tungsten lamps are used by them as the source of light, and two kinds of commercial glass approximating in their absorptive action cobalt blue and signal green are used as screens. In order to correct for the pronounced band of yellow-green transmitted by the cobalt blue, a film of gelatine dyed with rozazeine is also used. Although according to comparative measurements made by Ives and Luckiesh the light thus gotten is the closest approximation to average daylight yet obtained, still it shows a deficiency of 15 per cent. in the green and about 25 per cent. in the blue. Moreover the spectrum of this light does not show the brightness distribution of the spectrum by daylight. Since the absorbing screens cut down the light emitted by the tungsten lamp to 15 per cent. of its original intensity, the spectrum of the light finally given out shows the brightness distribution characteristic of lights of low intensity. We seem thus compelled either to give up the investigation of the color sensitivity of the retina for daylight illumination, or to devise some means of keeping this illumination constant. At an early stage of our work of standardizing the factors extraneous to the source of light, we were compelled to take into account the influence of the changes in the illumination of the visual field upon the color observation. The changes of illumination that took place from day to day, the progressive changes during the day, and the many sudden changes even in the course of an hour, rendered any constancy, or close reproduction of results entirely out of the question.

D. McF., 'A Standard for Color Values,' *Transactions of the Illuminating Engineering Society*, 1910, IV., p. 224.

[1] Ives, H. E., and Luckiesh, M., 'Subtractive Production of Artificial Daylight,' *Electrical World*, 1911, LVII., pp. 1092–1094.

In order to obtain a standard illumination, two things are necessary: (*a*) A means of controlling the illumination must be provided, which is sufficiently sensitive to cause small changes. (*b*) A method of measuring the illumination produced has to be devised; at least, a means must be secured for determining when an illumination has been obtained that is equal to a given preceding illumination. It is the purpose of this paper to describe an optics-room provided with means of control which we have found adequate to meet the above requirements; and to state a method of identifying and reproducing any given illumination of this room.

II. Description of Optics-Room

The dimensions of the room are 12½ × 10 ft. It is situated on the upper floor of an isolated building and is lighted by a skylight 8 × 7½ ft. Beneath the skylight two diffusion sashes, 4 × 7½ ft., are swung on hinges so that they can be raised or lowered as desired. The framework of these sashes is made of light-weight iron. For convenience of local control of illumination, if needed, each sash is divided into four units by means of cross-pieces. The sashes are filled with double-strength glass ground on one side, so adjusted to the frame that they can be removed easily for cleaning or for the substitution of some other kind of glass in case that is desired. This glass diffuses the light so effectively that local shadows cast by the cross-pieces in the framework of the skylight are completely eliminated, while the sudden changes of illumination produced by the passage of the sun behind a cloud are reduced to a minimum. This diffusion seems to have the further advantage of reducing the yellowness of direct sunlight below the limen of sensation. At least, when working under the sash, the observer never judged a gray exposed through the campimeter opening as yellow under any local conditions, as frequently happened when working under direct sunlight.

The room is planned also so that small changes of illumination can be produced, ranging from the intensive illumination of a south-exposure skylight to the blackness of a moderately good dark-room. Two provisions are made for this. (1) The

diffusion-sashes are made so that any or all of the panes of ground glass can be quickly and easily taken from the sash, and anything can be substituted that is desired; or the illumination can be varied by placing layers of tissue paper above the glass. (2) The room is provided with two curtains mounted on heavy spring rollers. One is a white curtain made of thin muslin; the other is a black light-proof curtain so mounted that, when drawn, its edges are deeply enclosed in light-proof boxing extending along the four walls of the room. One or both of these curtains can be drawn any distance that is desired, and the illumination can thus be changed gradually from a very intensive brightness to a fairly good blackness. To aid in getting dark-room effects, the doors of the room are carefully boxed and curtained. One requirement of a perfect dark-room, however, is lacking, namely, the walls and floor of the room are painted white. This is because it is of advantage in the light-room work, and because complete blackness is not needed in the type of work for which the room is devised.

III. Method of Standardizing

As stated earlier in our paper, no satisfactory means of determining the amount of daylight illumination in a room has been provided by the physicist, so there is little hope at this time of solving the problem from that side. The brightness induction of the peripheral retina, however, has been found by us to be extremely sensitive to changes in the general illumination. This phenomenon seems to provide us with a sensitive measure of these changes while, at the same time, it represents the combined effects for sensation of the principal subjective factors that might vary from day to day.[1] To apply the method in its most sensitive form, the inductive power of white was chosen because it is the most strongly affected by illumination changes. For example, when No. 14 Hering gray was used as stimulus and white as campimeter screen, a noticeable change was produced in the induction when the white curtain of the optics-room was pulled forward 1 cm.[2]

[1] This means of identifying the illumination of a room was devised by Rand.

[2] The sensitivity of this method of detecting changes in the general illumination was compared with the sensitivity of the Sharpe-Millar portable photometer. In

from a position in which its edge was directly above the long axis of the campimeter. This caused a change in the illumination of the room so small that it could not be directly sensed. Further, at 11 o'clock in the morning of a bright day in September, when a point at 25° on the nasal meridian was stimulated, one of the writers (Rand) reported that the white screen induced black across the stimulus No. 14 gray to an amount that caused it to equal in brightness 107° of black and 253° of No. 14 gray; at 2 o'clock of the same day the induction was increased until the No. 14 gray matched 150° of black and 210° of the gray; at 4 o'clock of the same day the No. 14 gray matched 180° of black and 180° of the gray.[1] Working at 25° in the temporal meridian, this observer reported at different times during one day and on different days, the wide variations shown by the following figures: 283° of black, 225°, 145°, 190°, 238°, etc. Another observer (Miss Campbell) reported less induction, but her variations from time to time were equally great. At 25° in the temporal meridian, she found at different

this photometer one of the comparison fields is illuminated by the light of the room and the other by a standard tungsten lamp enclosed in the photometer box. When the room is illuminated by daylight, the field receiving the light of the room is seen as white while the field lighted by the tungsten lamp appears as a saturated orange. The difference in color between the two fields renders the photometric judgment difficult and makes the instrument very insensitive for daylight tests. For example, our tests showed that by the method for identifying an illumination described in the text, a change in illumination could be detected which was produced by drawing the white curtain 1 cm. from a position in which its edge was directly above the long axis of the campimeter. But with the receiving surface of the portable photometer in precisely the same position as the stimulus screen of the campimeter, the edge of the curtain had to be moved 11.3 cm. in order that the change of illumination might be detected. Moreover, this amount of change could be detected only in case the photometric field was continuously observed while the curtain was being drawn, in which case the comparison field was observed to be slightly darkened. The judgment was made, then, in terms of a just noticeably different brightness of the field which was illuminated by the daylight, rather than in terms of a disturbance in the brightness-equality of the two fields. When, on the other hand, the judgment was made in terms of a just noticeable disturbance in the equality of the two fields, as the judgment would have to be made if the photometer were to be employed for the reproduction of any former illumination taken as standard, the curtain had to be drawn 44.2 cm. before the change could be detected. This j. n. d. represents an amount of illumination equal to 2.5 foot-candles.

[1] This increase in the inductive action of the screen caused by the decrease in illumination, was accompanied by a shrinkage of the zones sensitive to color covering an area of 4° to 6°.

times 80° of black, 103°, 160°, 175°, etc. After a careful study of the phenomenon with different screens and with different backgrounds, the inductive action of the white screen upon a stimulus of No. 14 Hering gray, at 25° in the temporal meridian, was found to provide the best means of detecting changes in the illumination of the optics-room. At this point on the retina, the induction was by no means minimal, nor was it sufficiently great to cause the medium gray chosen for our stimulus to appear too dark to give a small j. n. d. of sensation. Having thus provided ourselves with a means of producing small changes of illumination and a method of detecting them, we had in order to complete our work but to choose an illumination for each observer, which could be used as standard. Since we wished to work on both light days and days of medium darkness, an average had to be chosen as our standard from the measurements obtained on a number of days ranging from light to dark, so that on bright days, the room could be darkened, and on dark days it could be lightened until this value was obtained. For observer *A* (Rand) an illumination was selected which caused an induction of black across no. 14 gray stimulus viewed at 25° in the temporal meridian to an amount which caused the gray stimulus to equal in brightness 210° of black and 150° of no. 14 gray; for observer *B* (Ferree) 180° of black and 180° of no. 14 gray; and for observer *C* (Campbell) 145° of black and 215° of no. 14 gray. The amount of black induction was identified in each case by means of a measuring-disc made up of sectors of black paper and no. 14 gray of the Hering series. This measuring-disc was carried by a motor and placed just behind the 25° point. The observer fixated the 25° point and compared the gray of the measuring-disc as seen in central vision with the gray of the stimulus seen 25° from the fovea. The sectors of the measuring-disc were then changed until the two grays were of equal brightness.

Previous to each series of observations the illumination of the room was changed until the amount of brightness induction was brought to the value chosen as standard. It was tested at intervals during the sitting and was readjusted when

necessary. Details of the method of doing this are as follows. When the white screen and the no. 14 gray stimulus had been put in place, the observer took his position and adjusted the fixation-knot in front of the motor for the 25° point on the temporal meridian. The measuring-disc set at the standard value was mounted on the motor. The observer reported whether the stimulus appeared lighter or darker than the measuring-disc, or of a brightness equal to it. If the judgment lighter or darker was given, the curtain was drawn one way, or the other until the stimulus accurately matched the measuring-disc in brightness.

This method not only gives a sensitive measure of the changes of illumination of the visual field and a successful means of standardizing the illumination of a room by daylight, but it has in addition advantages for work in psychological optics not possessed by an objective standardization, could that be successfully obtained. The problem of standardization includes more for the psychologist than it does for the physicist, for the former has variables to take into account in addition to the changes that may take place in the energy of the stimulus. Even though the illumination of the room be made objectively constant, we should expect variations in the response of the retina to this illumination because of its own changes from time to time. Brightness contrast, for example, might be expected to vary from sitting to sitting even when the stimulus conditions are kept absolutely constant. Two factors would be concerned in these variations: changes in the inducing power of the surrounding parts of the retina, and changes in the sensitivity of the local area. These changes would take place even when the usual precautions known to the experimenter in this field have been observed. Such precautions are commonly limited to fatigue, adaptation, etc. These precautions do not provide for the changes that occur in the retina from day to day. Moreover, they do not adequately guard against the variations to which they are intended to apply, for no precaution can adequately guard against a change in a factor, unless some measure of that factor be had. So far as the writers know, in these general

precautions intended to keep the state of the retina constant, no measure of the variable factor has been provided to test the adequacy of the method. The method proposed by us, however, is planned with this in view. It takes into account not only the objective, but the subjective variables, and reduces both to a constant. For example, when no. 14 gray surrounded by the white field is made equal to the measuring-disc composed of 210° of black and 150° of the no. 14 gray for observer *A*, it means that the observation may be begun with the assurance that the total result of all the factors—the illumination of the room, the local sensitivity of the retina, and the inductive action of the surrounding parts of the retina —is the same as in the preceding observation.

What has just been said should not be considered as more than a general statement of the application of the principles of the method. In actual practice a greater refinement of working may be attained. If, for example, one wishes to use a preëxposure differing in brightness from that of the colored stimulus, and doubts whether a test which covers only the local sensitivity of the retina and the inductive action of the surrounding parts is a sufficient check upon the after-image sensitivity, he may make his standard include the effect of the preëxposure he wishes to use. In short, if he does not consider adequate the more general test we have described, he may duplicate, in establishing his standard, any combination of brightness factors, due to preëxposure, brightness of screen, or what not, that he may wish to use in his experiment proper.

The test of a method is how well it works. The test of this method is that we shall be able closely to duplicate our results from sitting to sitting regardless of the changes in the outside illumination from day to day or from morning until afternoon. The method stands the test. Long series of observations in the peripheral retina show a very small M.V. —much less even than is shown in the ordinary color observations in the central retina, where, as compared with the peripheral retina, the factors extraneous to the stimulus exert little influence.

The following table has been compiled from a number of observations to show the variations in the results of color limens and color limits (*a*) when the general illumination was controlled according to the method described above, and (*b*) when no more precautions were observed than were used by previous investigators. In previous investigations of the color sensitivity of the peripheral retina, care has been taken to work only at the same hours of days that appeared equally bright, or, if on days of different brightness, to make a rough approximation of preceding illumination by means of curtains without using either a definite standard or means of measuring. For our work with the illumination controlled, the gray of the brightness of the color at the illumination selected as standard was used for the preëxposure and the campimeter screen. For the work without especial control of the illumination, the gray of the brightness of the color for one of the days selected as typical was used throughout for preëxposure and screen. This gave in the first case complete elimination of the effect of preëxposure and surrounding field, and in the second case elimination as complete as could be gotten without accurate control of the general illumination. Results are given in the table for blue and green only because the sensitivity to these colors is affected most by changes of illumination.

Stimulus	Illumination	Screen and Preëxposure	Variations of Limits on Different Days	Variation of Limens on Different Days
Green.....	Controlled.....	Gray No. 9	0°	0°[1]
	Uncontrolled.....	Gray No. 8	4°–6°	60°–82°
Blue.....	Controlled.....	Gray No. 33	0°	2°–3°[2]
	Uncontrolled.....	Gray No. 32	4°–5°	18°–30°

[1] The limen for green was taken in both cases at 25° on the temporal retina.

[2] The limen for blue was taken in both cases at 40° on the temporal retina.

THE SENSATION OF MOVEMENT

BY JOHN E. WINTER

University of Michigan

A number of psychologists and physiologists have undertaken the study of the location of the sensation of movement in its various phases with varying results. Among those who have reached different conclusions, in greater or less degree are Nagel, Strümpell, Goldscheider, Angier and Pillsbury. Goldscheider[1] reached his conclusions after experimenting along three different lines: (1) Moving the finger by means of weights, (2) passing a current through the joints, and (3) rousing reflexes by means of tapping on the joint, applying acids, etc. (1) He experimented on the index finger by means of weights. The hand was placed in a static position with only the end of the finger free. A cord fastened at one end to the tip of the finger was passed over a pulley and a delicately poised scale was attached to the other end. By putting weights on the scales the finger could be moved until the subject detected the sensation. A needle attached to the scales recorded on a revolving drum the results of the experiment. The pressure of the string on the finger naturally produced a skin sensation, but Goldscheider asserts that in addition to this sensation another sensation was also recognized, easily distinguishable from the skin sensation, and located apparently in the joints. (2) To substantiate this result further experiments were made with an electric current passed through the fingers at different places, particularly the joints, with the result that when the electrodes were placed over the joints sensitivity was in every case reduced greatly. (3) He then experimented on the reflexes in frogs and rabbits. He attempted to produce a reflex in frogs by stimulating the surface of the hip and shoulder

[1] GOLDSCHEIDER. Ueber den Muskelsinn und die Theorie der Ataxie. *Gesammelte Abhandlungen*, 9–96. Ueber die Empflichkeit der Gelenkenden. *Ibid.*, 282–287

joints. An ordinary contact stimulus gave no reflex, while a light tapping on the joint produced movement, although Goldscheider himself admitted that in tapping other factors might have entered in to produce movement. Acids which did affect the adjacent skin had no effect whatever on the joint. With the rabbit the breathing reflex was used as an index of sensitivity but no uniform results were obtained. The skin of the leg was cut open to lay bare the bone and experiments were made on the periosteum, capsule and marrow. A light stimulus applied to the joint produced no reaction and when a stronger stimulus was used it sometimes resulted in reactions and sometimes not. Thus it might be inferred that sensation can be produced by mechanical means in the joint but it is not thereby proven that the joint is the seat of sensation. Reflexes were also produced by stimulating the periosteum and the marrow.

Goldscheider came to the conclusion that sensations of movement arise (1) from the rubbing of the articular surfaces and the wrinkling of the capsule, (2) strain on the tendons of one set of muscles and relaxation of their opposites, (3) change in the form of muscles. This opinion was very generally accepted until the first conclusion was criticized by Pillsbury[1] who proved first, that by passing a current through the wrist there followed nearly as marked a decrease in sensibility as when the current was passed through the elbow itself; and second, that the sensory innervation of the joint has not been definitely proven by histologists.

Angier[2] experimented with the lower arm and confined its movements to a horizontal direction. His problem was to determine the accuracy of judgment in comparing the distances of points by movements of the elbow. The details of his work are irrelevant to our discussion but some of his inferences are of interest. Angier believes with Goldscheider that sensation of movement comes largely from the joints. The condition of the muscle, whether normal, flexed or extended did

[1] PILLSBURY. Does the Sensation of Movement Originate in the Joint? *Am. Journ. of Psych.*, 1901, **12**, 346–353.

[2] ANGIER. Die Schätzung von Bewegungsgrössen bei Vorderarmbewegungen. *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, 1905, **39**, 429–448.

not materially affect his results, nor did the position of the arm within certain limits play an important rôle. In my experiments, however, I found that the angle of the arm and its position with reference to the body profoundly influenced results.

Strümpell[1] is inclined to ascribe the chief sensation of movement to the muscles, but he gives no reason for his opinion. He proved, however, that muscles could stand great lesions without disturbing the accuracy of perception. This would seem to combat his own theory, though it is not necessarily a disproof.

Nagel's[2] opinion is that sensations of movement cannot be derived from the skin. As Pillsbury believes that sensations of movement come from the tendons and muscles, the conflict seems to be chiefly between the rival theories, the joint theory and the muscle and tendon theory.

In Pillsbury's experiments little attention was paid to introspection but conclusions were drawn from an interpretation of physical results supplemented by an appeal to well-established physiological data as to the location of sensory endings, In our experiment there was also a minimum of introspection, the subject being merely asked occasionally whether he felt movement in any particular place. In one set of experiments, however, no heed was given to limina and the subject was asked to concentrate his whole attention in an endeavor to locate the place of sensation.

The following investigation was conducted at the suggestion of Professor W. B. Pillsbury, to whom as also to Dr. J. F. Shepard, the writer is indebted for suggestions and criticisms.

The subjects were Professor Pillsbury (*P*), Dr. Shepard (*S*), Messrs. Work (*W*), Woodrow (*Wo*), Cook, Osborn, and the writer (*Wi*), all of whom had had previous psychological training. Experiments were confined to measurements of the limen of movement of the elbow, normal and with current through the upper and lower arm, elbow, wrist and hand.

[1] STRÜMPELL. Ueber die Störungen der Bewegungen bei fast vollständiger Anasthasie. *Deutsche Zeits. f. Nervenh.*, 1903, 23, 1–38.

[2] NAGEL. Die Lage-, Bewegungs- und Widerstandsempfindungen. Handbuch des Physiologie des Menschen, III., 733–806.

The apparatus was the same as that used by Professor Pillsbury in his experiments in 1900. Since Spearman[1] criticized Pillsbury on the ground that the speed was too slow, four speeds were used. A hinged board served to support the arm which was raised by a cord passing over a pulley in the top of an upright. The cord was attached to one of a series of pulleys which were connected with a worm gear and run by means of an electric motor. A pointer in the end of the board passed over a millimeter scale on the upright and served to measure the amount of movement of the arm.[2] In all the experiments the subject sat with his arm resting upon the board, the elbow near the hinge. The angle of the elbow varied in the successive experiments from 75 to 150 degrees.

As indicated in the tables the first series of experiments were taken with the arm normal; then a series was taken with an induction current running through the elbow, wrist, tips of fingers, palm of hand or muscles and tendons of the lower arm. A series was also taken using ether for anæsthesia in place of an electric current. The apparatus was arranged so that the downward movement could be measured as well as the upward movement. The figures in the tables represent the averages of experiments ranging in number from 10 to 210.

When all was in readiness the motor was started and the operator gave a signal about two seconds before the board began to rise. The signal was given at this time in order that the subject might concentrate his attention to catch the first sensation of movement. A few experiments were taken without signal to test the subject's error of anticipation.

Of the four speeds used the first was the slowest, the second a little faster and the fourth was the fastest of all. A comparison of these speeds in terms of seconds is given below (Table IV).

A study of the tables given herewith will reveal among other things the following:

1. *The Influence of a Particular Angle.*—In Table I. the

[1] SPEARMAN. Fortschrift auf dem Gebiet der Psychophysik der räumlichen Vorstellungen. Tastsinn. *Arch. f. d. ges. Psych.*, 1906, **8**, 1–51.

[2] The length of the arm board from hinge to pointer was 52.4 cm.

angles for each respective set of experiments were uniform and hence allow of no generalizations. In Table II. *Wo*'s results in general, both normal and with current, in speeds 1 and 2, going up, show a limen for angle 135 to 150 degrees about twice as large as the limen for angles 75 to 80 degrees. In speed 4, this judgment is reversed, and in speed 3 the results obtained on different days are so various that other factors, physical or mental, must have entered. The results going down vary apparently without any relation to the size of the angle.

Wi's results in speed 2, going up, also show a larger limen for the larger angles.

TABLE I

	Elbow Angle		Speed		Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
	Up	Down			Up	Down	Up	Down	Up	Down	Up	Down
				Tests	210 mm.	20 mm.	50 mm.	20 mm.	40 mm.	20 mm.	50 mm.	20 mm.
W.	113	91	1		4.7	3.0	7.2	4.0	17.5	7.4	7.1	4.9
	113	91	2		5.2	3.5	6.0	3.2	16.8	5.0	6.0	3.0
	113	91	3		5.5	3.5	7.1	3.9	19.0	6.8	7.6	5.5
	113	91	4		4.6	3.7	5.8	5.0	11.8	6.0	5.9	4.6
				Tests	60	30	20	10	20	10	20	10
P.	123	51	1		4.9	6.9	5.3	5.6	14.3	6.1	13.3	6.9
	123	51	2		5.4	6.1	7.3	5.7	13.0	4.8	10.0	8.6
	123	51	3		6.8	4.6	9.0	4.3	15.3	5.6	12.2	7.6
	123	51	4		4.8	4.9	13.5	6.5	9.2	4.0	9.6	14.7
				Tests	20	10	10	10	10		10	
S.	135	117	1		2.8	5.9	8.9	9.8	6.1		4.5	
	135	117	2		4.6	4.0	6.8	9.5	6.0		5.3	
	135	117	3		5.6	4.7	8.1	9.2	7.5		6.0	
	135	117	4		5.7	4.8	7.1	8.3	5.6		3.9	

Speeds 1 and 3 partly reverse this order, but a new factor enters here,—the position of the arm with reference to the body. With the lower arm parallel to the line of the chest, angles 75 to 80 degrees gave a smaller limen than angles 135 to 150 degrees; but with the arm at right angles to the chest and with the elbow close to the body, the sensitivity with the small angles was greatly reduced. This was undoubtedly due to the extra pressure of the muscles against each other at the elbow. Inasmuch as the joint is little affected by the position of the arm in these experiments and the muscles are greatly

affected and the sensitivity reduced, the results would seem to corroborate Professor Pillsbury's theory of muscular sensation.

TABLE II

	Tests	Elbow Angle		Speed	Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
		Up	Down		Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.
Wo.	15	150	135	1	12.5	8.3	6.7	7.6	12.3	8.3	11.2	4.0
	18	75	75	2	5.3	6.1	10.3	4.0	16.8	6.2	10.5	6.0
	15	135	135	1	13.0	2.5	15.3	3.6	11.2	4.4	11.2	6.0
	18	180	80	2	6.3	4.5	8.1	3.0	7.7	4.9	7.5	7.2
	18	130	130	3	6.3	9.0	5.3	9.9	6.7	9.9	7.5	4.8
	18	130	130	3	15.6	5.2	23.1	15.7	28.	17.	30.5	5.4
	17	115	140	4	4.6	4.1	5.4	8.1	6.4	7.9	5.8	7.3
	18	130	130	4	3.1	5.4	2.5	5.7	4.4	2.7	3.1	4.0
	18	130	130	4	3.8	2.2	4.1	2.6	3.6	2.8	2.4	4.5
Wi.	15	130	130	1	6.9	2.6	16.4	2.5	28.6	11.6	30.6	9.4
	18	60	60	2	15.4	2.6	27.5	4.5	15.9	7.5	19.8	6.5
	15	130	150	1	17.9	6.3	35.1	10.8	30.0	16.8	29.7	13.4
	18	80	80	2	8.5	3.2	35.1	16.2	15.0	13.5	28.2	9.6
	15	150	130	3	6.3	4.6	12.8	10.5	22.8	17.1	22.2	15.0
	18	88	88	3	25.2	8.8	44.1	15.8	55.2	7.9	43.2	18.3
	18	85	85	3	18.1	4.2	43.7	9.6	48.6	6.9	51.7	7.5
	15	150	150	4	8.6	8.8	11.2	10.8	17.8	20.4	18.3	15.8
	18	175	176	4	9.1	7.1	19.3	9.3	24.2	6.2	25.8	9.4
	18	85	85	4	8.9	3.0	22.2	7.4	16.4	8.3	39.0	10.4

2. *Comparison of Ups and Downs.*—*Wo*'s results for speeds 1 and 2, going down, have predominantly the smaller limen. This is true of all of *W*'s results, of *P*'s speeds 2 and 3 and my speeds 1 and 2. With the other speeds the results either vacillated or were reversed. *S*'s results show that for normals the up movements had the larger limina and when the current was applied the down movements had the larger. Thus there seems to be little uniformity and the results warrant no generalization.

3. *Factors Influencing Judgment.*—(*a*) Accuracy of judgment is greatly influenced by the physical condition of the subject, and also by the peculiar position of the body. With the head leaning forward on the hand the limen immediately increased, as it did also when the head was allowed to hang forward. Remarkable differences in results, using the same angle and same position but on different days, are seen in *Wo*'s figures for speed 3, the limen ranging from approximate equality to a ratio of 4 to 1.

(*b*) The time of giving the signal. This is an important factor. Suggestion plays a strong rôle. The subject soon gets into the habit of expecting to feel movement at a certain time after the operator says 'now' and this causes large errors of anticipation. If the signal is given at a stipulated period of time, known to the subject, it is impossible to tell exactly just how many judgments are real judgments of movement. The number of zeros or mistaken judgments revealed by the records by no means tells the whole story. Often the subject was sure of movement sensation when the board was perfectly still. Occasionally the unevenness of the electric current was responsible for errors. Anæsthesia by means of ether obviates this difficulty to some extent. But if the subject was deceived once when he was positive he felt motion, why is it not possible that every other judgment of movement was only the result of imagination? This applies particularly to speed 1 which is so slow that even when the subject knows his arm is moving he frequently is unable to detect the sensation.

Again, in speed 1 we experimented to ascertain whether the sensation remained constant after movement had been (apparently) felt, and we found that sensation was not continuous but would be lost to consciousness at intervals and after a few seconds reappear. This indicated, then, a fluctuation either of attention or of the sensation of movement. This phenomenon offers an interesting problem to be worked out, although its presence detracts largely from the reliability of the results as a basis for scientific inferences regarding the location of kinæsthetic sensations.

Giving the signal about two seconds before the arm rises affords a uniform and constant expectation and the error of anticipation is perhaps reduced to a minimum. Varying the interval between signal and rise of board from one to ten seconds increases the error of anticipation. Giving the signal just as the board begins to rise vitiates the experiment as it scatters the attention for the moment, during which time movement might have been felt. The same is true if the signal is given shortly after the board begins to rise. Experiments show that under these conditions the limen rose considerably. The

constant gradual motion had already begun before the subject fixated his attention and there was no period of change from no-motion to motion in his attention.

(*c*) Degree of attention is also an important factor. Occasionally when the subject had been giving several judgments whose limen was found to be much larger than ordinary, he would 'buckle down' and the limen would immediately decrease.

(*d*) Elimination of the signal. A number of experiments were taken without giving a signal,—the operator at first following the old method of allowing the board to rise or fall at stated intervals, and later holding the board from 1 to 90 seconds. The results are here given.

TABLE III

	Tests	Elbow Angle		Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
		Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.
Wo. . .	15	145	80	5.5	8.0	6.2	4.8	6.5	4.0	9.9	3.2
Wi. . . .	18	160	85	21.1	9.0	26.7	16.0	26.8	15.7	50.8	19.0

Naturally the long waits increased greatly the error of anticipation, and many erroneous judgments were made. The figures which show only the averages really give a deceptive idea of the experiment. In reality the limen varied from zero judgments to 88 millimeters. All of my results without signal were much larger than those with signal, but *Wo*'s limen without signal was as small as the smallest of the others. He seemed all along to be possessed of very keen sensitivity. My results show that several times, after waiting as long as from 50 to 90 seconds, I yet made a wrong judgment.

4. *Comparison of the Speeds.*—Spearman (**6**) criticized the low speed used in Professor Pillsbury's experiments. To obviate this difficulty we used four speeds whose rates were as follows:

TABLE IV

Distance traversed, 0 mm. to 10 mm.

Speed	1	2	3	4
Seconds	13.34	5.24	4.30	1.90

Speed 1, it will be seen, is very slow, the board moving only ten millimeters in thirteen seconds, while speed 4 is rapid. The results obtained with speed 1 were the least satisfactory, especially when a current was used. The subject would frequently say, "I think I feel movement but am not sure," and even when the subject knew his arm was moving he frequently could not detect sensation.

5. *Location of the Seat of Sensation.*—Anæsthetizing one part of the arm is sure to result in a partial anæsthesia of the neighboring parts by a kind of irradiation. This complicates the problem, as we cannot tell just how large a share of the effect we are to attribute to the anæsthesia of the particular part in question. Acting on the general principle, however, that the part to which the current is applied will be most affected by it, we may say that to establish Professor Pillsbury's theory from experimental results it will be necessary to show that when the current is applied to the muscle, or the union of muscle and tendon, the sensitivity is reduced as much as when applied to the joints. *W*'s records show in practically every case that when a current was applied to the muscle of the arm the sensitivity was less than with the current applied to either the wrist or elbow. *P*'s record, going up, generally coincides with *W*'s. His records going down show no uniformity. In *S*'s case, the sensitivity was most reduced when the current was passed through the elbow, and in *Wo*'s case and my own there is absolutely no uniformity.

A comparison of the limina with current applied to the wrist and elbow, in succession, fully substantiates the contention of Professor Pillsbury that anæsthesia of the wrist causes almost as great a decrease in sensitivity as anæsthesia of the elbow. In several cases the limina for the wrist were greater than those for the elbow. A series of experiments were taken also with a current applied to the upper arm, and it is significant that the limen thus obtained was about twice as large as the limen for the normal experiments.

It has seemed to the writer that introspection during the experiment should play a larger rôle, and that its results would be more conclusive than the objective study of limina. In our

experiments the subject was sometimes asked where he felt sensation, and the answer showed that most of the sensations were located in the finger tips and hand, while scarcely any sensations were felt in the joint or muscle. *Wo* insisted throughout that his sensations were nothing but skin sensations, and in the experiments conducted as those described above I incline to the same view. With the up movements there was a sensation of 'pushing up' from beneath, and with the down movements there was a kind of 'sinking' feeling throughout the hand. With regard to this sensation it may be well to note that if they are in reality skin sensations they are entirely irrelevant to the problem and should be ignored; for it is certain that they would not be felt if the arm were raised without the aid of the board underneath. If, on the other hand, these sensations are more deeply seated, though seemingly located in the skin, the probability is that they are muscle and tendon sensations stimulated by the contraction of the muscles and tendons at the elbow with which they are intimately connected.

To obtain a little more introspection, a series of experiments was taken in which the subject was asked to attend, not to the moment *when* sensation was felt, but *where* it was felt. Almost every conceivable answer was given, showing conclusively that mere introspection could never solve the question as to the location of sensation. Here are a few of the answers given: skin and muscle; under wrist; elbow; finger tip; wrist to finger; muscle at elbow; whole arm; arm and shoulder; muscle under arm; near wrist; skin at elbow, etc., etc.

The last series of experiments were the most interesting and, I believe, the most fruitful of results. It was found in the preceding experiments that running an electric current through the finger tips and the ball of the thumb did not result in perfect anæsthesia of these parts. The sensation was still located there more than anywhere else. To make sure of the anæsthesia of these parts, as well as the skin under the muscle, ether was applied on sponges with the result that no sensations were felt there. The introspective results are noteworthy. The subject announced that he no longer had a skin sensation

of any kind but that a different sensation was now felt (different in kind) and located very perceptibly in the muscle and tendon. It would seem that these results come nearer a solution of the problem than the former ones. Ether produces an unnatural state of the organs, to be sure, but not so unnatural a state as the electric current produces. The current produces motion through the whole arm and increases the difficulty by compelling the subject to discriminate between movement caused by the rising board and that caused by the current. The element of false movement is at least eliminated with the use of ether, thus giving a better opportunity for introspection.

Before giving a summary of our results it might be well to mention the statement of Dr. Shepard that most of his laboratory students in the last ten years, several hundred in all, have obtained results similar to those given here.

If we bring together the results of the experiments, it seems that the effect of passing a current through the elbow is not as Goldscheider assumes to anæsthetize the surface of the joints but to anæsthetize the muscles and possibly the tendons about the joint. That this is true is seen from the fact that a similar current passed through the muscles anywhere else has the same effect, and in much the same degree. The current passed through the upper arm, the forearm, or the wrist tends to increase the limen and in many cases more than when passed through the elbow. This conclusion is confirmed by introspection. The sensation of movement is almost without exception assigned to the fingers, muscles of the forearm and other muscles or tendons or the surface of the body over them. It might be noticed in this connection that the illusion of pushing up at the elbow when mercury is poured from a beaker or when a heavy weight is lowered upon a cushion is always referred to the muscles below the elbow, not to the joint, and is to be explained from the sensations in the muscles themselves, not to the pressure that they exert upon the joint surfaces. In fact there is no single bit of real evidence that the sensations of movement come from the joint surface. This experimental result is in harmony with the statements of the histologists that there are no sense endings

on the joint surfaces. The sensation may come from the capsule, from the ligaments and from the muscles and tendons that are involved in the movements but not from the joint surfaces.

Summary

1. Passing a current through the wrist reduces sensitivity as much as passing the current through the elbow. This would seem to refute the evidence offered by Goldscheider for proving the joint to be the seat of movement sensation.

2. An apparently better way to indulge in introspection is to use ether to anæsthetize the skin.

3. The results obtained with the use of ether tend to corroborate Professor Pillsbury's theory that the muscles and tendons are the seat of the sensation of movement.

MIND AS MIDDLE TERM

BY ROBERT MACDOUGALL

New York University

Into much recent discussion there enters, in some aspect or other, the controversy as to what place the mental system shall be accorded in psychological science. The problem reappears in many guises and general statements as well as particular working conceptions have been formulated regarding it. The preliminary chapters of text-books customarily define the author's position in the matter, and in numerous papers and addresses of late special modifications of the psychological conception of mind and reconstructions of the limiting criteria by which the field of investigation is determined have been made.

The older psychology was not troubled by doubt in regard to these matters. It defined psychology in terms which gave a precise formal limitation to its field; and since, supported by philosophical and theological assumptions, the distribution of its subject-matter was restricted to mankind and approached by a purely introspective method, the maintenance of a clear demarcation of its province from that of adjacent sciences presented no great difficulty. The relation of consciousness to physical structures and changes was incidental not essential. The soul in its temporary alliance made use of the body as an instrument, but in action as well as quality was ultimately independent of the latter. It was not considered in reference to either a determining stimulus or a necessary reaction. In itself the mind composed a unity of functions and the object in studying it was to determine the place of each of these functions in a rational system. Psychology was thus concerned with the logical problem of the mind's constitution and its direction in ideal activity.

In this conception of mind as a self-contained system of phenomena whose limits are stated strictly in terms of consciousness psychology has concurred in its more recent defini-

tions as well. The formulations with which current text-books introduce their subject-matter adhere to this postulate. Psychology is the science of self and the facts of self as manifested in individual experience; it is concerned with psychical phenomena, conscious processes or psychoses; the description and explanation of the phenomena of mind or consciousness is its aim. In these mental activities and conscious states as such psychological interest centers. Its business is the systematic exploration, under the methods which inductive science imposes, of the constitiuton of mind and its internal correlations.

This substitutes an empirical study—systematically directed introspection under experimental control—for the logical reflection upon which earlier rational psychology depended. Nevertheless its field is defined in similar terms, as the system of psychical activities or phenomena. The psycho-physical correlations which may exist, whether conditioning or dependent, are incidental to the discussion. The study of stimulus and reaction may be helpful in many ways to psychological science, but with neither of these, if we adhere to the implications of such current definitions, is the latter directly concerned. It is the reaction in consciousness which follows external stimulation or physiological change in the one case, and in the other the mental complex which, generically or in particular, precedes a given form of reaction that alone affords material for psychological study.

But in the more recent development of the science this conception has undergone a variety of modifications due in part to natural changes accompanying the extension from within of the field of psychological investigation, and in part caused by external pressure through the study, from the standpoint of independent sciences, of those phenomena with which the correlations of mind bring it into contact. The movement from within has been a complex one. Its most obvious constituent is to be found in the development of comparative psychology and its extension both of the experimental method of study and of the guiding conceptions of psychology until by a succession of rapid strides the whole animal kingdom from man to protozoan had been included within its field of research.

The enormous multiplication of individual types to be studied, in which this extension has resulted, is accompanied by a still more profoundly modifying factor, namely the extreme qualitative variety of the forms of consciousness to which in the course of his work the psychologist must adapt his conceptions. The simple and precise formulæ which served the earlier human and classic psychology fall to pieces when the psychic life of microörganisms is to be included along with the complex and highly articulated consciousness of man in a common system. This problem has been met, by the psychologist himself, in a variety of ways. To one phase he has responded by simplifying and universalizing the essential constitutents of the unity of consciousness, as when the existence of irritability, discriminative selection and adaptive reaction, demonstrated in the simplest organic types, are construed as a manifestation of the psychological trinity—affection, cognition and volition. To another aspect of the problem he has reacted by substituting for this unitary system of common characters the conception of individual mental functions, such as sensitivity, organic memory, space-orientation, learning by imitation, and the like, the evolutionary history of each being traced as the succession of organic forms is passed in review. In a similar way the comparative psychologist has modified his working conceptions to meet still other demands imposed by the continuous extension of his field.

A second general constituent in this modifying process is to be found in the complication of phenomena by which the psychological student is confronted within the limits of human experience itself, or of the human type. In this field extension has taken three general directions. The first is from the normal through the exceptional, abnormal and pathological to the final disintegration of the unitary self in individual impulses, elementary idea-systems and persistent reaction-types. This field has hitherto been the most productive of such supplementary modifications. It has occasioned the conceptions of subliminal or subconscious phenomena, of psychic disaggregation and split-off selves, of motor and psychic automatisms, with a host of other working hypotheses.

The other extensions lead, the one downward through subnormal and defective types, imbeciles and idiots, towards a limit which the anencephalous monster may be taken to represent; and the other backward from adulthood to youth and infancy, and from fœtal to embryonic conditions until, in the fertilized ovum, it meets the comparative psychologist on common ground in dealing with a simple undifferentiated organism. The need of devising, in the service of continuity, an adequate system of conceptions thus receives a new and greater emphasis.

All these demands have arisen from within in the course of the psychologist's own work; but to the readjustments by which they have been met he has been urged by an independent and extraneous stimulus. This also has had a complex character, covering both systems of physical change with which mental activity is correlated, the field of the stimulus and that of the reaction.

The study of the physical conditions of consciousness, especially of its physiological locus, has been approached in independence of any primary interest in mental phenomena. While the psychologist has availed himself largely of the results both of physical and physiological research in the technical arrangement of his problems as well as in the correlation of results, he has not been hampered with any confusion, as to aims or methods, between the general province of physics and that of his own special studies. The science of optics, for example, is not confounded with the study of space perception nor climatology with psychic reaction to weather changes. In the case of physiology, however, the uniformity of association between the primary series of reactions in nervous and other tissues and the mental activities with which the psychologist deals has led to the inclusion of the latter group of facts within the system of phenomena which, in the most general sense, is to be considered. For physiology these mental reactions can never become an independent system coördinate with the neural processes, and the account it gives of them assumes their dependence upon physiological changes throughout. Thus the psychosis is conceived not as a psychological object but merely

as one product of nervous action, and its treatment constitutes only a highly specialized topic within physiology at large.

The study of sensory stimulation in all its forms and of the mechanism of simple and complex reaction-types has been so extensive in recent years and the mass of detailed information it has afforded concerning psycho-physiological correlation has been of such importance to psychology that physiological methods and conceptions have attained dominance in this field even with psychologists. A sketch of the nervous system and its functions is made the introduction to psychological study; mental processes are explained in terms of nervous habits and rearrangements; in general, a treatment of the direct psychological relation of experience to the external world as condition and object of the will's reactions is replaced by speculative constructions of its physiological relation to mediating processes in the central nervous system. These modifications in the psychologist's working conception often involve more than a reformulation of criteria and amount to a plain confusion between the standpoints of independent sciences.

The second of these two external influences proceeds from the biological study of reactions. The biologist not infrequently makes use of the physiological method in his work, and a rigid application of his own postulates would perhaps require the application of this conception throughout; but, as distinguished from the latter, ecological biology is concerned not with the special mechanisms of reaction but with their teleological relations. It construes the response of the organism in terms of its serviceableness to some end, and its object is to determine the complex of adaptations which thus characterizes the systematic reactions of any given type or individual. Each organism maintains certain permanent relations with the environment. Its energies are directed to securing food, shelter, warmth, protection and alliance—in a word, provision for the satisfaction of certain needs and desires. Each reaction may therefore be conceived in terms of its approximation to the realization of those conditions which determine the maintenance of these relations in an ideal form. Many such adaptive reactions we know to be pervaded by

consciousness; to a much greater range we impute the same general character to them, and it may perhaps be questioned whether the terms in which the biologist defines his object have, in strictness, any meaning apart from the implications of consciousness and its values. But at least the biologist does not start from this assumption. Reaction consciously directed to an end is but a special form of response having its place in a more general field of organic adaptation with which, as a whole, the science is concerned. Biology, in this phase of its work, is the science of behavior, whether behavior be construed in terms of consciousness or not.

In recent years, as the systematic study of life in its ecological relations has extended, it has been brought into more intimate as well as more extensive contact with the system of phenomena which functional psychology considers in its study of mental adaptation. Especially is this true in the case of comparative psychology where method is necessarily objective. These two sciences may be said to have come face to face in the study of animal behavior. The results in this field have been similar to those already pointed out. The difficulty of securing a satisfactory criterion for determining the distribution of consciousness, especially in view of the apparent variability in its association with a given function, and the sense of identity in the fundamental nature of behavior in all organic species has led to modifications not merely in the formulæ applied to particular types of life but to a recasting of the terms in which the subject-matter itself is stated—for example, when the scope of psychology is defined as a study of 'organosis' in its most general application.

In this new approximation towards a neighboring science the primary conception from which procedure starts is again objective, but instead of the conditioning stimulus it is now the consequent reaction which becomes the determining element. Behavior must always be considered, it need not be said, as well as physiological function and external stimulus. The mind is historically and socially conditioned in reaction as well as incitement, and its materials of expression must be regarded equally with its provoking stimuli. This modifica-

tion, however, goes farther than to employ the form of response as a means of interpreting the subjective attitude. It proposes a study of the objective rearrangement in its effects upon the conditions of life as a substitute for the inquiry into the forms of conscious activity by which, under certain limitations, such readjustments are characterized.

In still other connections the same general question as to the nature of the system of facts with which psychology is actually concerned has been raised. One of these may be used to point the consideration which all in common provoke, since it is not dependent upon the complex extensions of recent psychological investigation into fields which bring it into contact with the physical and biological sciences but has been introduced as a comment upon the earlier classic method of introspection by the normal human mind. It is the query concerning what is actually meant by the terms mind and experience, mental facts and mental laws, as psychology conceives them.

In this particular modification of the central psychological conception mind is construed as the system of characteristics and habits which the individual presents in his reaction to stimuli. It is what the mind does, not what it is, which is here considered; and what it does is expressed in its attitudes and social reactions. What comes under review by the psychologist, according to this conception, is not the form and conditions of the mental activity as such but its logical and practical aspects, its products and consequences. These lie open in some degree to even the casual observer, and the intimate companion of any man is in a position to make a comprehensive system of judgments concerning the character of his mind in this sense of the term. It is thus we learn the habits and character of the individual, the range and accuracy of his knowledge; by observing the plans he has formed and carried out or given up we judge his originating and organizing capacity and his tenacity of purpose; similarly, we may know his predilections in a multitude of affairs and be familiar with his general tastes and desires.

But it may be questioned whether in the existence or

acquisition of such knowledge the essential attitude of the psychologist is at all embodied, for it rests primarily upon the determination of relations between two objective series, that of the stimulus and that of the reaction; and leaves in doubt the place of the intermediate system of mental activites from which the psychologist takes his departure. Now there are two distinct points of view from which mind or experience may be conceived; the one regards the qualitative, the other the relational aspect. The first of these standpoints is sometimes called the subjective, the second the objective point of regard.

Under the first conception immediate, irreducible experience is intended, the fact, namely, that existence has at each moment a unique qualitative character which constitutes it a moment in the concrete history of an individual subject. In its raw immediacy one thus knows yellow, noise, cold and pain; one feels satiety, longing and dislike. With this character a second subject cannot be brought face to face, nor can it be shared with him. The experience of another simply is not, and cannot become, my experience; no adequacy of constructive interpretation, no sense of sympathetic intimacy, no accordance in social reaction will annul this fact. My mind is not obscured from the view of other minds, when I conceive mind in this way, because of the complexity of its workings or the deviousness of its course, but because—to continue the figure—it is not at all a visible object. It is hidden because it is inaccessible. To know another mind, in this sense of the word, is to be that other; that is to say, it is to deny the fact of otherness and to bring the event in question within the category of immediate experience. Whatever the status of this qualitative aspect of existence in reference to any specific problem, and whether it concerns the psychologist's work or not, the uniqueness and exclusiveness of subjective immediacy in each individual experience is a fact to be recognized, not a theory to be discussed.

From the second standpoint mind is treated in terms of its relations to the objective world. Whatever the qualitative aspect of any experience it springs from certain stimuli and results in specific reactions. It is in these physical and social

connections that the observer is interested. His point of departure is in a system of reality lying beyond the experience of the moment and his return is to that larger world again. The mind, in this case, is but the point where a stimulus has effect and a reaction is originated. To know it means to be acquainted with the characteristic response which is made by it to any situation. When this response is conceived in terms of the physical reactions necessary to the maintenance of life and of the social adaptations of which our fellow-men can take account, it is not essential that the qualitative nature of the experience as it exists for the subject should be taken into account. In the reckoning which the observer makes the mental system may be ignored, for it is the characteristic reactions to which it leads in their objective and social forms alone in which he is interested. So long as his knowledge of the sequential connections between typical stimuli and the responses which the individual makes to them is secured the immediate quality of experience which mediates between the two series is negligible.

Such knowledge may even be more exact and complete when the observer is a bystander than when he is himself the subject of the experience. The occupancy of the locus of experience in no way ensures an acquaintance with the real character of the mind in this sense of the term. One's estimate of his own capacities may be farther astray than that of the impartial onlooker, and his reaction in any given case may surprise him as really as his acquaintance. It is indeed the latter to whom we look for a sound judgment in regard to such a matter, for he is undisturbed by that emotional excitement which is inseparable from personal experience and unoccupied with attention to the purely subjective aspect of the situation from which the experient can never wholly free himself.

If we regard the mind from this objective standpoint it is obviously neither inaccessible nor hidden. One's character is recognized as widely as acquaintance extends. To one person it is known less fully than to another; to one this series of reactions is more familiar than that, and thus individual estimates of character vary; but to all alike the data for such knowledge

are accessible, the subject himself having simply the position of one observer among an indifferent many.

But this way of dealing with reactions is obviously defective, whether it be considered practically or theoretically, since knowledge of the situation involved is incomplete in regard to stimulus and reaction alike. For the stimulus is not the object as it exists for the onlooking individual or is objectively defined; it is the object as presented in the experience of the subject himself. The reaction, similarly, is not the gross physical movement or socially discernible adaptation; it is the whole attitude of the self aroused by the situation which is thus presented. A full description of the stimulus in physical terms is indeed conceivable, but this cannot rest upon any analysis of the constitution of the external object alone; it must include the organic reaction which this stimulus provokes and thus be finally stated in physiological terms. The reaction, likewise, if its full description in physical terms is attempted, must be conceived as the whole complex readjustment, peripheral and central, which the physiological stimulus has evoked. That such a multiplicity of elements of physical change exists, occurring in the body at large and constituting a physiological analogue to both the complex mental situation which is presented and to the reaction in consciousness which it occasions, is a methodological assumption and not a field of data accessible throughout its range and utilizable by the observer in making up his account. This holds true also in regard to practical affairs, for the onlooker is constantly driven to recognize the insufficiency of his knowledge of the real stimulus to which response is made, and the incompleteness of his acquaintance with the reaction itself.

Thus even when the observer's interest lies wholly outside the limits of individual experience the subject's report of any situation is indispensable to the completion of his data. If he had possession of all the facts regarding either the physiological effects of the stimulus or the final readjustment within the body which it arouses he might be able to predict the reaction upon the physical or social environment, but such knowledge is inaccessible and the only alternative is to find how the situation

presented itself to the subject in question and what his real and complete response to it was, whether such response resulted in the immediate production of changes observable by the onlooker or not.

The psychologist's standpoint cannot be identified with either of the points of view above described. Each individual experience possesses a subjective quality which is at once unsharable and indescribable. It bears also certain relations to both antecedents and consequents in the external world which it is practically important that the subject and his fellowmen alike should understand. But the psychologist is occupied neither in demonstrating the qualitative uniqueness of each individual experience nor in tracing its practical consequences in the form of movements. His standpoint is subjective but not qualitative; it is relational but not objective. The first of the two points of view above described is subjective and qualitative, the second is relational and objective, while that of the psychologist is subjective and relational. His study is of facts which cannot be objectively discerned, though their existence may under certain conditions be inferred from objective data, but the facts thus revealed through intuition he treats in terms of their relations, in whatever direction these relations lead him.

In general the plexus of connections in which any individual experience stands may be treated in terms of a three-fold grouping. The first of these is the relation of antecedence, in which the experience is studied in connection with its conditions, whether these lie within the course of previous experience or derive from the external world in the form of so-called stimuli. The second is the relation of reciprocity, including the material study of the constituents of each individual experience and the formal study of resemblances and correlations among the phases of such experience. The third and last is the relation of consequence, in which the influence of the event upon both the course of subsequent experience and the forms of expression by which mental activity is characterized are studied.

The second of these three groups is, by definition, restricted

to the immediate phenomena of subjective experience. It conforms most closely to the conceptions and methods of the earlier introspective psychology. In contemporary science the observation of it is systematically controlled and extended statistically. In comparison with the traditional conception of mental constitution its scheme has consequently undergone both complication and reconstruction; but in general data and products this part of psychology remains essentially unmodified. It is an analysis of the psychic system itself to determine its constituents and the forms of their combination in the various orders of synthesis by which mind is characterized. Concerning this part of the psychologist's work, therefore, disagreement is not likely to arise.

It is in the first and third of the foregoing sections, in which the two-fold correlation of consciousness with its physical environment is treated, that the danger exists of obscuring if not obliterating the fundamental conception which both defines the subject-matter and determines the limits of psychology. In these fields of study, quite as much as in connection with that central system of facts to which normal introspection has been directed from the beginning, consciousness with whatever that term implies must remain the final point of reference if psychology is to have any independent existence. Otherwise its province will simply be parted between physiology which invades its field from the side of the stimulus and biology which encroaches from that of the reaction. For psychology these correlations of consciousness are necessarily secondary and contingent. To assume either correlative as a dominant conception, that is, to define the province of investigation in terms of the stimulus-field or of the reaction-system, carries one beyond the circle of psychic phenomena into the world of physical materials and their changes, and to combine the two as is the tendency in much contemporary writing makes of psychology a pseudo-science created merely by taking slices from two independent sciences and combining them. The maintenance of psychology rests upon a clear definition of its aim as a science and a perception that the system of consciousness presents a substantial and unitary subject-matter which

cannot be dissipated in a confused treatment of individual topics in physics, anatomy, physiology, biology and anthropology.

If, however, just this solution of continuity is to be avoided the centrality of consciousness must never be lost sight of. This thesis has two points of application; the first touches the substantial existence of the mental system as the primary field of psychology, and the second concerns its primacy as an interpretative criterion in the treatment of its physical correlations. In the first place, then, the objective point of view is not homologous with the standpoint of psychology. Practical interest seeks only adaptation, whether it deal with things or men. It is concerned not with the mechanism of any change in itself but with its products or effects, and when it has to deal with minds it treats them in accordance with this general aim, classifying all reactions in terms of their relations to stimuli which constitute their nearest antecedents in the discernible series of changes which the objective world presents.

If now we can say that a given stimulus inevitably arouses the mental reaction in question and from such a mental reaction these physical consequences and no others proceed, the middle term which is thus repeated may be dropped from the series and the first and last terms connected directly. Towards this general conclusion all the conceptions above described tend. The field of actual transformations is reduced to the two physical systems and their contact is marked only by a theoretical division. But in the system of reality mind is not a mere point where stimulus and reaction meet, as these various modifications imply when carried to their logical conclusion. It is psychologically an interposed system in which the stimulus-field terminates and the reaction-system has its origin; and it is the existence of this mediating system which constitutes both the ground and limitation of the science. This interposition, as already indicated, implies no interruption of continuity; it is not a metaphysical solution but a methodological subdivision of reality which it involves.

Consciousness, in other words, does not possess an independent field which can be contrasted with that of stimulus

and reaction loci. It not only has a physical correlation in the physiological system of activities but may even be described as the flooding of these two fields with ideal values and direction. Nevertheless it both constitutes in itself a definite and complex system of phenomena and affords the only means by which an approach can be made to the problem of stating in its fulness the nature of either stimulus or reaction. This system, therefore, instead of receding to the vanishing-point becomes for psychology the central field of exploration within which repeated and extended analysis, by indirect as well as by direct means, reveals an endlessly increasing complexity and integration.

The second application of this thesis is in the psychologist's treatment of the physical changes with which consciousness is correlated whether as antecedent or consequent. It may be stated by saying that the conception of consciousness and its implications affords the determining reference which both defines the field to which the psychologist limits his activity and supplies the qualitative criterion which guides his work. The systematic reference of consciousness, as already indicated, is to the stimulus-field on the one hand and to the reaction-system on the other, and it has no third and independent theater of activity. But in the psychological treatment of these two groups of phenomena consciousness must remain a determining conception. The stimulus enters within the circle of consideration only when it ceases to be regarded in terms of physical change and is treated as the antecedent of a specific qualitative consciousness. The reaction, likewise, becomes subject-matter for psychology only when it is no longer conceived as a movement or material reconstruction but is construed as the embodiment of a particular mental attitude.

In both of these cases the situation, as psychologically conceived, is made to turn upon the presence of consciousnses as its cardinal point. The stimulus is that which provokes mental activity, the response that which expresses it. Thus it assumes as its foundation the existence of affective sensibility and conative tendencies, of hedonic values and preferential reaction in the organism which thus responds to stimulation. To con-

sider irritability in the physiological meaning of the term alone, or reaction as organosis in the general sense of an adaptation which is not based upon consciousness is to relinquish this constitutive assumption and to make the implication of consciousness an incident in a larger system. But for the psychologist the elimination of consciousness in this way is simply pouring out the baby with the bath, and in all valid extension of his science or its underlying assumptions it will be found on closer inspection that this conception still functions.

The psychologist does indeed study the whole system of specific stimuli, whether physical or physiological, which acts upon the senses, as well as the characteristic reactions which the organism makes to them; but these are never, as a matter of fact, construed by him in thoroughgoing mechanical terms. It is the reactions of living creatures in which he is interested and the stimuli he considers are those only to which such organisms are irritable. Every force may logically be called a stimulus or reagent and every substance upon which it impinges may be said to present a reaction in the rearrangement of its physical relations which follows the collision. But no one advocates a modification in our conception of mental science which will make it coextensive with this whole field. Such an extension appears only in the metaphysical universalization of concepts, with which psychology as a special science has nothing to do.

Even within the field of organic life itself a division obtains between activities which are conceived to fall within the limits of psychological phenomena and those which are excluded from consideration. The former are not restricted to reactions dependent upon the coördination of many individual muscle groups, which can be construed only as reflecting the unity of the organism as a whole, but include also certain adaptive responses mediated by single organs and directed to the readjustment of the relations of that organ individually. Reflex action may be taken to represent this group. This conception, however, is not extended to the whole range of changes occurring within the organism. Absorption, osmosis and the chemical syntheses of nutrition are excluded from the

circle of phenomena which psychology treats. If the question be raised why these forms of reaction, together with such activities as capillary attraction and the selection and rearrangement of materials in the growing crystal, are thus excluded from consideration the distribution will be seen to turn upon the fact that in the case of these physical and metabolic reconstructions it has been found possible to treat the phenomena in purely mechanical terms as movements and combinations depending upon physical forces alone.

In its most extended form psychological treatment is thus still restricted to the sentient world. It is consciousness, in its most elementary forms indeed, but still consciousness, which determines where the line shall be drawn and not mere readjustment in the relations which characterize the system of physical materials. If any of the forms of change enumerated above, osmosis, absorption, etc., are included by any particular scientist it will be found that for him these processes are either steeped in consciousness at the moment of their occurrence, as for example, the activity of the organic cell at large has been conceived, or they are regarded as permanent forms of reaction the development of which has been mediated by consciousness in the past.

The system of habits represents this problem generically. The psychological student finds it necessary to contrast habitual reaction with the selective activity of consciousness. The formation of habit is marked by the progressive decline in directive attention. In its more established types it has already passed beyond the field of choice and control, while its theoretical limit is a complete dissociation from conscious activity, a condition which is at least approximated in the so-called vital functions, digestion, circulation and the like. The automatisms of habit therefore present in the highest degree the phenomena whose treatment these psychological extensions have been designed to serve. They are highly specialized adaptations in which the response to stimulation is direct and simple, depending upon no interposed activity of consciousness. As organic reactions they have teleological significance but they are independent of a psychical correlate.

If the field of psychology is to be redefined in terms of biological adaptation instead of mental process habit-automatisms seem to constitute at once the immediate occasion and sufficient justification for the change.

Nevertheless habit, which is tenaciously retained within the sphere of psychological discussion, maintains its place simply in virtue of the necessary relation to the selective and organizing activities of consciousness which is predicated of it. Though any given reaction of this type be now dissociated from consciousness and bring about by purely mechanical processes a teleological readjustment to changes in the external world it still logically falls to the psychologist to discuss it if it be thus construed as a product of consciousness, namely as a permanent reaction-type developed through the selective and organizing reactions of an antecedent mental activity. In this construction of habit, in which contemporary psychology at large agrees, the centrality of consciousness is maintained, for the habit-form is viewed in the light of psychic values and direction.

The application of this conception in the progressive extension of the field of psychological data is by no means restricted to the immediate reactions of the organism. It determines that growing system of investigations into culture and social history which depends upon the interpretation of permanent products of human activity, such as the monuments of literary and plastic art, or the industrial inventions and general material transformations which have been brought about in the service of mankind. These are legitimate fields of psychological inquiry because—and only because—of the implications of consciousness by which their treatment is everywhere suffused.

Wherever this underlying principle is applied the psychological point of view is assumed and the phenomenon becomes a datum for the science. It must therefore be said, I believe, that those investigations in which organic stimulation and reaction have been studied by other than physicochemical methods involve the implication of consciousness, whether carried on by physiologists and biologists or by psy-

chologists themselves, and must in consequence be classed as psychological inquiries in the strict sense of the term. In some cases doubtless the application of this principle is due to confusion, but in others the scientist is under no illusion as to the nature of his work. Psychological science was first laid under obligation to physiology in this field, and that the debt is great as well as obvious a mere list of its contributors sufficiently indicates. More recently the tide has turned in the direction of biology whose students now hold the same general relation to experimental psychology which physiology possessed a generation ago. It may be assumed that an equal enrichment of the science is to be expected from this side also, and one which will react to the advancement of biology and its conceptions as physiological psychology has influenced the study of physiology. But in this general extension of knowledge it would be the very irony of fate if psychology were to lose sight of those distinctive conceptions upon which her existence as a science rests through a failure to apprehend the fact that all this constitutes primarily an enrichment of her own system of data, and that without such a fundamental reference to the forms and values of consciousness it can have no logical existence.

DISCUSSION

THE CASE AGAINST INTROSPECTION

It is rather generally agreed among English psychologists that there is something (state, process, act, relation, or whatever) which may properly be called *introspection*. There is also rather general agreement in the definition of the term, whatever may be said of divergences in regard to its practical application. The greatest disagreements have been over the temporal nature, the difficulty, and the reliability of 'introspection.'

It is now high time that we should question, more seriously than has been done heretofore, the existence of 'introspection' in the traditional sense. It is for this purpose necessary to present some actual usages of the terms 'introspection' and 'consciousness' in English psychology, although it is not at all necessary to go over the whole field of psychological writings and cull every instance in which use has been made of these terms. The discussion of the uses of *Selbstbeobachtung*, *Bewusstheit*, and other German psychological terms, is an entirely different piece of work which may or may not be profitable; it certainly is not profitable in English and I have no intention of engaging therein.

'Introspection' is usually defined in terms which are equivalent to the expression *consciousness scrutinizing itself*. Such definitions are significant only when 'consciousness' and 'scrutiny' and 'itself' or whatever terms are substituted for them are more explicitly defined. Typical statements from psychological texts are given below.

James says: "It means of course the looking into our own minds and reporting what we there discover. Every one agrees that we there discover states of consciousness" ('Principles,' I., 185). Angell: "It consists simply in the direct examination of one's own mental processes" ('Psychology,' 4th ed., 5). Judd: "In observing this conscious state, he introspects." Stout: "To introspect is to attend to the workings of one's own mind" ('Manual,' Introd., Ch. 2, 2). Stratton: "This direct acquaintance with the state of our minds which all of us to some extent possess" ('Experimental Psychology,' 2). Yerkes, in discussing 'introspection': "It is by observing my own consciousness that I directly study the objects of consciousness"

(Introduction, 41). Maher, the exponent of Thomism: "States of consciousness can only be observed by introspection—that is, by the turning of the mind in on itself" ('Psychology,' 4th ed., 11).

The technical use of the word 'introspection' in this way is of recent introduction (see Oxford Dictionary). But the signification is very old. We need not pursue it back farther than Reid, Hamilton, Bain and James Mill, to get a definite understanding of the extent to which 'self-consciousness' is involved in British theories. The discussion here runs into the consideration of the term consciousness, to which we must give a little space.

Bain[1] distinguishes and lists 13 different senses in which the term was used. The catalogue is now too short, for James's usage of the term does not belong anywhere in it. With the greater number of the uses we have no great concern. We should point out, however, that Reid made of consciousness a separate faculty, practically the 'introspective' observation of the modern psychologists (First Essay, Chapter 1). Hamilton while having some agreement with Reid in the use of the term, contended that consciousness is involved in every mental act: "Can I know without *knowing* that I know? Can I desire without *knowing* that I desire? Can I feel without *knowing* that I feel? This is impossible. Now this . . . common condition of self-knowledge, is precisely what is denominated consciousness" ('Metaphysics,' Lect. IX., p. 110, in American ed. of 1880. The whole of this lecture is especially important).

What we now call 'introspection' is described by Hamilton as follows: "In an act of knowledge, my attention may be principally attracted either to the object known, or to myself as the subject knowing: and in the latter case, although no new element be added to the act, the condition involved in it—*I know that I know* becomes the primary and prominent matter of consideration" (Lecture XI., p. 135).

In strong contrast with the use of the term 'consciousness' by Reid and Hamilton, we find James Mill declaring: "To say I feel a sensation is merely to say that I feel a feeling, which is an impropriety of speech. And to say that I am conscious of a feeling is merely to say that I feel it. . . . In the very word feeling all that is implied in the word consciousness is involved ('Analysis,' Ch. V.). To which Bain felt constrained to add a footnote correcting what he considered a serious error.

The modern views of 'introspective' consciousness are best represented by the statements of Stout and James, because these

[1] 'The Feelings and the Will,' 4th ed., 538, *et seq.*

two have made the attempt to work out a system in which 'introspection' is not only admitted, but is really provided for. I shall confine my discussion therefore to these two authors. Other introspectionists have simply claimed that 'introspection' occurs without trying to show the nature or details of the process.[1]

In Stout's writings there is less confusion between consciousness (in the cognitive aspect, at least) and the objects of consciousness, than in the writings of other psychologists. "Psychical states as such become objects only when we attend to them in an introspective way. Otherwise they are not themselves objects, but only constituents of the process by which objects are recognized" ('Manual,' p. 124). "The object itself can never be identified with the present modification of the individual's consciousness by which it is cognized. This holds true even when we are thinking about modifications of our own consciousness. The conscious experience in which we think of another conscious experience is always at least partially distinct from the conscious experience of which we think" (pp. 58–59). If we confine our discussion for the present to the realm of sensational consciousness, we find that the objects which the sensation cognizes are 'sensible qualities' (p. 57) or 'sensory elements' (p. 120).

The 'sensible quality' red, and the sensation of red, one would think, differ in that the redness is in the quality or is the quality; the sensation should have no redness, for it is an element in the process of perceiving red. This is apparently what Stout means, so far as the sensation is primarily concerned. But the sensation has the property of becoming secondarily an object for another psychical state, and then, of course, it has objective qualifications. Obviously the only quality which we can consistently ascribe to the sensation of red in its secondary capacity is the 'sensible quality' it cognizes in its primary capacity: "If we compare the color *red* as a quality of a material object with the color *red* as a quality of the corresponding sensation, we find the redness as immediately perceived is an attribute common to both. The difference lies in the different relations into which it enters in the two cases" (p. 123). (See also footnote, page 58.) The sensation, as an object has intensity, as well as quality (p. 30), and when when referred to the physical world, is correlated with wave-length, and not with any 'sensible quality.'

Here we have the whole scheme of 'introspective' consciousness.

[1] This of course does not apply to those who explicitly hold to the scholastic doctrine of introspection. I hope to show in a later paper that in the scholastic doctrine of the intellect there is a good foundation for the doctrine of introspection.

A sensation, as such, is not an object, but the awareness of an object; hence it is not observable, but an observation. This Stout sees clearly, and grants freely, and so far we can go with him. But, demanding that the sensation shall be nevertheless observed (for what reason we shall see later), Stout assumes that the sensation which primarily is consciousness, or awareness, is, or may be, secondarily what it is not primarily, namely, an object for another awareness, which may be either subsequent to the first awareness or simultaneous with it (pp. 18–19).

We wonder indeed what the 'mind' is which 'one' attends to ('Manual,' Introd., 2) and we might indeed wonder what the 'one' who attends is: these apparently simple assumptions become exceedingly complicated and shaky when introspection is included. Surely the mind is not the mere sum of the processes, for we are told that "the most important drawback is that the mind, in watching its own workings must necessarily have its attention divided between two objects," implying that it is only one process after all which cognizes both objects; for that there should be any difficulty in one process cognizing one object and another process cognizing another object, whether the second object is or is not the first process, does not seem reasonable. Without question, Stout is bringing in here illicitly the concept of a single observer, and his introspection does not provide for the observation of this observer; for the process observed and the observer are distinct.

James's doctrine of 'introspection,' as stated in the *Principles*, is less inconsistent than Stout's. That James seriously doubted the actual existence of the machinery he built up in theory does not in any way lessen the need for its examination, since the influence of James's speculations concerning consciousness is unfortunately very strongly felt in psychology.

"There are realities, and there are 'states of mind,' and the latter know the former; and it is just as wonderful for a state of mind to be a 'sensation' and know a simple pain, as it is to be a thought and know a system of related things" (II., 5–6).

"The *relation of knowing* is the most mysterious thing in the world. . . . Knowledge becomes for him (the psychologist) an ultimate relation that must be admitted, whether it be explained or not" (I., 216).

Here is an unmistakable deviation from Stout. For Stout, the term 'mental process' applies to the *knowledge;* for James it is primarily the *knower*, and knowledge is assumed as an additional

process, with which James concerns himself little, although involving it freely in his system. "The passing Thought then seems to be the Thinker" (I., 342). This "thinker" knows external objects, and it also knows past thought.

"It may feel its own immediate existence—we have all along admitted the possibility of this, hard as it is by direct introspection to ascertain the fact — but nothing can be known *about* it until it is dead and gone" (I., 341).

'Introspection' is then for James, first, the knowing of the knower (not of the knowing), and secondly is always retrospection. The division of attention in regard to which Stout trips, comes in here however more legitimately. "The Thought, which whilst it knows another Thought and the Object of that Other, appropriates the Other and the Object which the Other appropriated" (I., 340) is manifestly doing double duty; is simultaneously observing two different things at once.

James and Stout agree in postulating an 'introspection' which makes objective that which is primarily non-objective, but differ in that while James is postulating the objectification of the subject, and not dealing at all with the *knowing*, although specifically postulating it in addition to the subject, Stout is postulating the objectification of the *knowing* and deals with a subject only illicitly.

The objectification of the subject is for James not an occasional matter, but an essential aspect of the functioning of the 'stream of consciousness.' "*The knowledge of some other part of the stream, past or future, near or remote, is always mixed in with our knowledge of the present thing*" (I., 606) although "A mind which has become conscious of its own cognitive function plays 'the psychologist' upon itself. It not only knows the things which appear before it; it knows that it knows them" (I., 272–3).[1] This psychologizing is apparently only a special development of the universal function of mind by which it preserves its unity through the present subject knowing or 'appropriating to itself' the past subjects.

The doctrine of the essentially retrospective nature of 'introspection' is very useful to James in defending the 'transitive' states of consciousness which he admits cannot be discovered by 'introspection.' "*For a state of mind to survive in memory, it must have endured for a certain length of time.* In other words, it must be what we have called a substantive state. Prepositional and con-

[1] I must confess that in the above quotations I find more 'mixed in with the knowledge' than James explains, especially in connection with the knowledge of the future, but I think the general meaning is clear.

junctional states of mind are not remembered as independent facts — we cannot recall just how we felt when we said 'how' or 'notwithstanding.' Our consciousness of these transitive states is shut up to their own moment — hence one difficulty in introspective psychologizing.

Any state of mind which is shut up to its own moment, and fails to become an object for succeeding states of mind, is as if it belonged to another stream of thought" (I., 643–644).

The essential points in James's scheme of consciousness are *subject*, *object*, and a *knowing* of the object by the subject. The difference between James's scheme and other schemes involving the same terms is that James considers subject and object to be the same thing, but at different times. In order to satisfy this requirement James supposes a realm of existence which he at first called "states of consciousness" or "thoughts," and later, "pure experience," the latter term including both the "thoughts" and the "knowing." This scheme, with all its magnificent artificiality, James held on to until the end, simply dropping the term consciousness[1] and the dualism between the thought and an external reality.

'Introspection' can hardly be bolstered up by James's mechanical psychology. To assume that the thought of a cabbage knows a feeling of regret, and that the thought of a cabbage may in another moment be known in turn by the thought of a red necktie, is ingenious but ineffectual. As the knower in the act of knowing is not known, but is known only after it has finished its cognizing, the assertion that what is now known was once a knower remains a mere assertion to the end. All that James's system really amounts to is the acknowledgment that a succession of things are known, and that they are known by something. This is all any one can claim, except for the fact that the things are known together, and that the knower for the different items is one and the same. This further implication James does not escape, in spite of the assumption of a series of different thoughts assuming the knowing function, for after all, the knowing function is the same in each case; the thoughts all take the same point of view in knowing other thoughts or things and it is the point of view which constitutes the real I or subject.

The real claim to admission which 'introspection' holds in James's original scheme is therefore not based on the turning of a subject into an object, but on the existence of two sorts of objects. There are, according to James's 'Principles,' thoughts, which are known; and

[1] James, 'Does Consciousness Exist?' *Jour. of Philos.*, etc., I., 478; also, 'A World of Pure Experience,' *Jour. of Philos.*, etc., I., 538–541.

the things corresponding to the thoughts, which are also known. A cabbage is known, and there is also in the stream of consciousness a 'thought' of a cabbage, which is known, no matter by what. If this sort of representationalism is accepted, there is no objection to calling the knowing of the thought 'introspection' meaning therefore by the term exactly what Reid meant by 'consciousness.' But the day for such psychical mechanics has gone by. The ghostly world of representational 'ideas' or 'states of consciousness,' dim shadows through which we may look at the real objects casting them, or on which alone we may fasten our gaze, attracts no longer faith nor interest. It is significant in this connection that James, in giving up the term 'consciousness,' abandoned his whole representational scheme, without however giving up the essential mechanics of his doctrine of knowledge. Hence, for his last psychology, there is virtually no 'introspection' possible.

There are probably no psychologists at the present time who hold to 'introspection' explicitly on the representational grounds of Reid and the older view of James. If there are any such, I certainly do not wish to argue the point with them. For one who believes in representationalism a belief in representationalistic 'introspection' is quite the consistent thing.

I am inclined to suppose that the greater number of those modern writers who explicitly presuppose 'introspection,' have in mind, however dimly, the sort of 'introspection' which Stout defines.[1]

The objections to Stout's theory are not of the same order as the objections to the theory of James, although just as profound. There can be no denial of the existence of the thing (knowing) which is alleged to be known or observed in this sort of 'introspection.' The allegation that the knowing is observed is that which may be denied. Knowing there certainly is; known, the knowing certainly is not.

I may observe, or be aware of, a color, an odor, or any other sensation (sense datum); I may be aware of relations and feelings; I may be aware of any combination of these; but, Stout to the contrary notwithstanding, I am never aware of an awareness.

The possible objection to the statement just made, and probably

[1] See for example, in addition to the authors above quoted, Calkins, 'Psychology' (1910), 6–8. Myers, 'Experimental Psychology' (1909), 3–5. Pillsbury, 'Essentials of Psychology' (1911), 6–9; 'Attention' (1908), 212–217. Royce, 'Outlines of Psychology' (1903), 16–18. Titchener, 'Text-book of Psychology' (1909), 15–25. G. E. Moore, 'The Nature and Reality of Objects of Perception,' *Proc. Aristot. Soc.*, N. S., VI. (1905–6), 102–104. None of these authors explicitly presents a theory of introspection, so that we cannot say positively that they agree with Stout.

the logical foundation of the 'introspection'-hypothesis, is as follows: If one is not aware of awareness, he does not know that it exists. If one denies that he is ever aware of a thing, and that any one else is ever aware of it, he has no right to say that there is such a thing. The force of this argument is purely imaginary.

It may sound paradoxical to say that one cannot observe the process (or relation) of observation, and yet may be certain that there is such a process; but there is really no inconsistency in the saying. How do I know that there is awareness? By being aware of something. There is no meaning in the term 'awareness' which is not expressed in the statement "I am aware of a color (or what-not)."

So much for the logical foundation of 'introspection'; there is however a psychological reason for the rise of the theory. So many psychologists would not have assumed the reality of 'introspection,' if there were not some process or operation which simulates it. This process, I think, may be readily pointed out.

When one observes some 'external' object, as for instance sound, there are simultaneously present a number of other objects which are intimately connected with the observing of the sound, and which may not be themselves observed clearly. The muscular sensations from the tympanum, neck, breast, and other regions; the visual 'images'; the feelings; the visceral sensations; all these are definitely modified in the listening for the sound, and yet may not be vivid. On the other hand, the attention may be turned to these accessory facts, and the importance of the auditory sensation may be secondary. In this case, there seems to be a turning of the attention from the 'outer' fact (the sound) to the 'inner' facts. These facts are 'inner' in that they concern, or are constituents, of the body, or objective self. By a rather natural step, accordingly, these inner facts are taken to be the process of observing the sound. Observation of them is therefore the process of observing the process of observing the sound —*introspection*.

Stated in detail, this sort of 'introspection' is quite clearly the observation of things which are just as objective, considered from the point of view of knowledge, as is the sound; the trouble comes from the fact that we are apt to omit detailed statements. The double distinction between the subject and the object and between the self and the not-self, almost inevitably leads, in the absence of rigid analysis, to the identification of the objective self with the subject, and hence the vague conclusion that processes associated with the knowing of external objects are processes *of knowing* the same objects.

In actual practice, most psychologists who use the term 'introspection' and define it as the observation of consciousness not only do not seek to apply it in strict accordance with the definition, but they even apply it to the whole range of psychological observation. In giving 'introspective reports' on the observation of a sound, for example, the sound itself is usually included as one of the 'introspected' details. So colors, odors, after-images, and all other objects of consciousness, are quite commonly said to be 'introspectively' observed. This practice constitutes effectively the *reductio ad absurdum* of the 'introspection' theory. Starting as a distinctive kind of observation, the observation of an observation of something, it finishes as the only kind of observation. In other words, there would seem to be really nothing to observe except the observation of something else!

There is, as a matter of fact, not the slightest evidence for the reality of 'introspection' as the observation of 'consciousness.' Hence we must, in default of such evidence, cease the empty assumption of such a process. We might keep the word to apply to the processes we have described above (observation of feelings, and of kinesthetic and cœnesthetic sensations); a term by which to designate the observation of these factors would be very useful, and 'introspection' is the legitimate term for the purpose, since these factors are the real 'inner' ones of which psychology has been talking for so long a time; but in view of the word's quite disreputable past it is probably better to banish it for the present from psychological usage.

KNIGHT DUNLAP

THE JOHNS HOPKINS UNIVERSITY

NOTE. — After the foregoing discussion was placed in the hands of the Editor, Professor Titchener's interesting 'Prolegomena to a Study of Introspection' appeared in the July number of the *American Journal of Psychology*. Professor Titchener discards the Hamiltonian doctrine of the mind being 'self'-conscious in every cognition. What he substitutes for this doctrine is not made altogether clear, but apparently it is a theory similar to that of Stout or else (and this is more probable) the scholastic doctrine. This is indicated by such things as the implicit application of the term 'introspection' to the observation of sounds (p. 436), the statement that the psychologist 'is observing his own mind' (439), and the statement that 'introspection is the interrogation of experience' (440). The strongest indication is the contention that 'introspection' is not

necessarily a conscious process (442 *et seq.*). This doctrine, which at first seems highly paradoxical, is quite intelligible if we remember that 'consciousness' in Professor Titchener's mind-scheme is made up of 'processes' which are by no means to be identified with cognitions of objects, but rather with objects cognized. It is quite consistent with this terminology to say that 'introspection' is not primarily a 'conscious process'; it is the observation of a conscious process.

Psychological Review Publications

PSYCHOLOGICAL MONOGRAPHS

VOL. I.

1. *On Sensations from Pressure and Impact: H. Griffing. Pp. 88. 2. Association: M. W. Calkins. Pp. vii+56. 75 cents. 3. *Mental Development of a Child: Kathleen Moore. Pp. iv+150. 4. Kant's Psychology: E. F. Buchner. Pp. viii+208. $1.50.

VOL. II.

5. Problems in the Psychology of Reading: J. O. Quantz. Pp. iv+51. 75 cents. 6. The Fluctuation of Attention: J. P. Hylan. Pp. ii+78. 75 cents. 7. *Mental Imagery: Winfred Lay. Pp. ii+59. 8. *Animal Intelligence: E. L. Thorndike. Pp. ii+109. *9. The Emotion of Joy: George Van Ness Dearborn. Pp. ii+70. 10. *Conduct and the Weather: Edwin G. Dexter. Pp. viii+105.

VOL. III.

11. *On Inhibition: B. B. Breese. Pp. iv+65. 12. On After-images: Shepherd Ivory Franz. Pp. iv+61. $1.12. 13. *The Accuracy of Voluntary Movement: R. S Woodworth. Pp. vi+114. 14. *A Study of Lapses: H. Heath Bawden. Pp +122. $1.50. 15. The Mental Life of the Monkeys: E. L. Thorndike. Pp. iv+57. 50 cents. 16. *The Correlation of Mental and Physical Tests: C. Wissler. Pp. iv+62.

VOL. IV

17. Harvard Psychological Studies, Vol. I.; sixteen experimental investigations. Edited by Hugo Münsterberg. Pp. viii + 654. $4.00.

VOL. V

18. Sociability and Sympathy: J. W. L. Jones. Pp. iv+91. 75 cents. 19. The Practice Curve: J. H. Bair. Pp. 70. 75 cents. 20. The Psychology of Expectation: Clara M. Hitchcock. Pp. iv+78. 75 cents. 21. Motor, Visual and Applied Rhythms: J. B. Miner. Pp. iv+106. $1.00. 22. The Perception of Number: J. F. Messenger. Pp. iv+44. 50 cents. *23. A Study of Memory for Connected Trains of Thought: E. N. Henderson. Pp. iv+94.

VOL. VI

24. A Study in Reaction Time and Movement: T. V. Moore. Pp. iv+86. 75 cents. 25. The Individual and his Relation to Society: J. H. Tufts. Pp. iv+58: 50 cents. 26. Time and Reality: J. E. Boodin. Pp. v+119. $1.00. 27. The Differentiation of Religious Consciousness: Irving King. Pp. iv+72. 75 cents. 28. University of Iowa Studies. No. IV. Edited by C. E. Seashore. Pp. v+118. $1.25.

VOL. VII

29. Yale Psychological Studies, New Series, Vol. I. No. 1. Edited by Charles H. Judd. Pp. vii + 226. $2.25. 30. The Theory of Psychical Dispositions Charles A. Dubray. Pp. vii+170. $1.50. 31. Visual Illusion of Movement during Eye Closure. Harvey Carr. Pp. vi+127. $1.25.

VOL VIII.

32. **The Psychological Experiences connected with the Different Parts of Speech.** ELEANOR H. ROWLAND. Pp. 42. 40 cents. 33. **Kinæsthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze:** JOHN B. WATSON. Pp. vi+100. $1.00. 34. **Yale Psychological Studies: New Series. Vol. 1. No. 2.** Edited by CHARLES H. JUDD. Pp. v+197. $1.75. 35. **Studies from the Psychological Laboratory of Wesleyan University. Vol. I. No. 1. An Experimental Stu y of Visual Fixation.** RAYMOND DODGE. Pp. vii+95. $1.00.

NOTE—No. 36 appears as No. 1 of the Philosophical Monographs.

VOL IX

37. **Studies from the Psychological Laboratory of the University of Chicago. Control Processes in Modified Hand-Writing; An Experimental Study.** JUNE E. DOWNEY. Pp. vii+148. $1.50. 38. **University of Iowa Studies in Psychology. No. 5.** Edited by CARL E. SEASHORE. Pp. 148. $1.50. 39. **Studies from the Psychological Laboratory of the University of Chicago. Combination Tones and Other Related Auditory Phenomena.** JOSEPH PETERSON. Pp. xiii+136. $1.50.

VOL X

40. **Studies from the Johns Hopkins Psychological Laboratory.** Edited by G. M. STRATTON. Pp. 104. $1.00. 41. **The Social Will.** EDWIN ANDREW HAYDEN. Pp. iv+93. $1.00. 42. **Studies from the Psychological Laboratory of the University of Chicago: The Effect of Achromatic Conditions on the Color Phenomena of Peripheral Vision.** GRACE MAXWELL FERNALD. Pp. iv+91. $1.00. 43. **Wellesley College Studies in Psychology, No. 1. A Study in Memorizing Various Materials by the Reconstruction Method.** ELEANOR A. MCC. GAMBLE. Pp. xi+211. $2.25.

VOL XI

44. **Studies from the Psychological Laboratory of the University of Illinois, Vol. I., No. 1.** Edited by STEPHEN S. COLVIN. Pp. vi+177. $1.75. 45. **Ohio State University, Psychological Studies, Vol. I, No. 1;** Edited by THOMAS H. HAINES. Pp. 71. 75 cents. 46. **Studies from Psychological Laboratory of University of Chicago, An Experimental Study of Fatigue.** C. S. YOAKUM. Pp. vi+130. $1.25. 47. **Studies from the Johns Hopkins Psychological Laboratory. The Determination of the Position of a Momentary Impression in the Temporal Course of a Moving Visual Impression.** N. T. BURROW. Pp. 63 65 cents.

VOL XII

48. **A Study of Sensory Control in the Rat.** FLORENCE RICHARDSON. Pp. 124. $1.25. 49. **On the Influence of Complexity and Dissimilarity on Memory.** HARVEY A. PETERSON. Pp. 86. $1.00. 50. **Studies in Melody.** W. VAN DYKE BINGHAM. Pp. vi+88. $1.00. 51. **Report of the Committee of the American Psychological Association on the Teaching of Psychology.** Pp. 94. $1.00. 52. **Some Mental Processes of the Rhesus Monkey.** WILLIAM T. SHEPHERD. Pp. 66. 75 cents.

VOL XIII

53. **Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests.** Pp. 108. $1.00. 54. **Tests for Practical Mental Classification.** WILLIAM HEALY and GRACE MAXWELL FERNALD. Pp. viii+54. 75 cents. 55. **Some Types of Attention.** H. C. MCCOMAS, JR. Pp. 56. 75 cents. 56. **On the Functions of the Cerebrum: the Occipital Lobes.** SHEPHERD IVORY FRANZ and GONZALO R. LAFORA. Pp. 118. $1.25. 57. **Association Tests:** Being a Part of the Report to the American Psychological Association of the Committee on Standardizing Procedure in Experimental Tests. R. S. WOODWORTH and F. LYMAN WELLS. Pp. 86. 75 cents.

VOL XIV

58. **The Diagnosis of Mental Imagery.** MABEL RUTH FERNALD. Pp. 160. $1.50.

PHILOSOPHICAL MONOGRAPHS

VOL I

1. **Æsthetic Experience: Its Nature and Function in Epistemology.** W. D. FURRY. Pp. xvi + 160. $1.60. 2. **The Philosophy of John Norris of Bemerton.** FLORA ISABEL MACKINNON. Pp. iii+103. $1.00.

Price $4.00 the volume. *Monographs so marked are out of print.